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To

The Hon:ble

Lord Kaims

from the Author.

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PHYSIOLOGICAL ESSAYS,

CONTAINING,

I. An Inquiry into the Cauſes which promote the Circulation of the Fluids in the very Small Veſſels of Animals.

II. Obſervations on the Senſibility and Irritability of the Parts of Men and other Animals; occaſioned by Dr. *Haller's* late Treatiſe on theſe Subjects.

The Elder

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EDINBURGH:
Printed by HAMILTON, BALFOUR, and NEILL.
M,DCC,LV.

BRITISH MUSEUM
31 JAN 40

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I.

AN

INQUIRY

INTO THE

CAUSES which promote

The CIRCULATION of the FLUIDS

IN THE

Very SMALL VESSELS *of Animals.*

THE following Paper was read at ſeveral Meetings of the PHILOSOPHICAL SOCIETY *of* EDINBURGH, *in the Years* 1745 *and* 1746; *and is now publiſhed with ſome Corrections and Additions.*

BRITISH MUSEUM
31 JAN 49

AN INQUIRY INTO THE CAUSES which promote the CIRCULATION of the FLUIDS IN THE *Very* SMALL VESSELS *of Animals*.

ALTHO' the Circulation of the Blood has been almoſt univerſally acknowledged for above a century paſt, and much has been wrote in order to explain this doctrine; yet are there ſeveral things relating to it which have not been, hitherto, ſo ſatisfactorily accounted for, as to render any farther inquiry into them altogether ſuperfluous: and of this kind, we preſume, is the motion of the fluids thro' the ſmaller veſſels.

THE firſt authors who embraced the *Harvean* doctrine ſeem to have aſcribed the whole of the circulation, both in the arteries and veins, to the force of the heart *. But *Borelli*, in whoſe time it was believed by many, that the arteries and veins were not continued canals, but divided by an intermediate ſpungy ſubſtance, plainly ſaw, that, in this caſe, the blood could not be conveyed into the orifices of the naſcent veins, by any force of the arterial fluids puſhing it forward; and, therefore, he ſuppoſes it to enter them in the ſame manner as the particles of water inſinuate themſelves into a ſpunge or other porous ſubſtance: but as, in his days, the *phænomena* of capillary tubes were very little known, and the reaſons of them not at all underſtood, 'tis no wonder that, after declaring attraction to be an impoſſible thing, he aſcribes the above effects to the gravity of the fluid itſelf*; nor does he ſeem to have been ſufficiently aware, that, after water has riſen to a determinate height in ſmall tubes, or a certain quantity of it has been received into porous bodies, no more of it will enter into either of theſe.

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* Jo. Walaei epiſt. ad Bartholin. De motu chyli et ſanguinis.

Dr. *Pitcairn*, in his *Diſſertatio de Circulatione ſanguinis per vaſa minima*, after ſhewing that animal ſecretion cannot be performed by means of ferments in the glands, or by theſe bodies acting as filtres, endeavours to prove, that the various ſecretions from the blood are intirely owing to the different diameters of the ſecretory veſſels: but he makes it no part of his inquiry, by what *powers* the fluids are puſhed thro' theſe veſſels. However, that there might appear no difficulty in the motion of the fluids thro', even, the ſmalleſt tubes of the body, nor any ſuſpicion

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* Borelli, De mot. animal. pars 2. prop. 32.

of their ſtagnating in them, we have lately been told, that the blood moves more quickly in the ſmaller than in the larger veſſels; an aſſertion ſo inconſiſtent with the laws of hydraulics, when applied to the animal frame, that it could ſcarcely have been expected to have dropt from the pen of a writer much leſs noted than Dr. *Hoffman*[*].

But, how eaſy ſo ever it may have appeared to ſome authors to account for the motion of the fluids in the ſmall veſſels of animals, yet whoever impartially conſiders the reſiſtance that a fluid, moving thro' the *aorta* and all its branches, muſt meet with from friction, which increaſes as the diameters of the veſſels decreaſe, and adds to this the mutual attraction or coheſion between the particles of the fluids and the ſides of the veſſels in which they move, will not only ſee that there is, at leaſt, ſome difficulty in this matter, but be alſo apt to ſuſpect that neither the force of the heart, nor the alternate contraction of the larger arteries, is ſufficient to drive the fluids thro' the ſmalleſt veſſels of the brain, *teſtes*, and many other parts of the body.

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[*] Frederic. Hoff. ſyſt. med. l. 1. § 1. c. vi. No xvii.

In order, however, to ſet this affair in a clearer light, we ſhall particularly conſider the ſeveral cauſes to which the circulation of the blood has been commonly aſcribed.

SECT. I.

Of the force of the heart, contraction of the arteries, gravity and attraction of capillary tubes, conſidered as cauſes of the circulation of the fluids in the ſmall veſſels of animals.

The principal cauſe which propells the blood thro' the body, is, without doubt, the contraction of the heart: let us, then, firſt, inquire how far this may be ſuppoſed ſufficient to account for the motion of the fluids in the very ſmall veſſels of animals.

IF the force with which the blood is thrown, by the left ventricle of the heart, into the *aorta*, be ſuppoſed equal to the preſſure of a column of blood 90 inches high *; the *momentum* of this fluid in any artery will be found, by multiplying the *area* of the tranſverſe ſection of that artery into 90, the height of that column of blood, whoſe preſſure is ſuppoſed equal to the protruſive force of the heart: for the product gives the number of cubic inches or parts of a cubic inch of blood, whoſe weight is equal to the preſſing power with which the blood is driven by the force of the heart into that artery.

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* Dr. *Hales*, from a variety of experiments made on horſes, dogs, ſheep, and other animals, thinks it probable, that the blood would riſe ſeven feet and an half, or 90 inches, in a tube, fixed into the carotid artery of a middle-ſized man. Statical Eſſays, vol. 2. p. 40.

The diameter of a circulating red globule of blood, has been generally reckoned ſomething leſs than $\frac{1}{3000}$ part of an inch; but Dr. *Martine* has, from *Lewenboeck*'s and *Jurin*'s later obſervations, ſhewn it to be $\frac{1}{1933.5}$ part of an inch *; and *Lewenboeck* has obſerved, that one of theſe globules is ſometimes obliged, in paſſing thro' a very ſmall capillary artery, to change its figure into an oblong ſpheroid, ſo that the diameter of ſuch an artery may be ſuppoſed nearly equal to that of a red globule. If then, for the ſake of more eaſy computation, we ſuppoſe the diameter of a red capillary artery to be equal to $\frac{1}{2000}$ part of an inch, the *area* of its tranſverſe ſection will be 0.000 000 196, and this multiplied by 90 gives 0.000 0176 parts of a cubic inch of blood, which amounts to 0.00466 or $\frac{1}{214}$ part of a grain †;
and

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* Medical Eſſays, vol. 2. art. vii.

† A cubic inch of warm blood is reckoned by ſome 266, and by others a little more than
267

and is equal to the moment of the blood, ariſing from the force of the heart, in a capillary artery, whoſe diameter is $\frac{1}{1000}$ part of an inch, upon the ſuppoſition that there were no loſs of motion from friction, and that the areas of the tranſverſe ſections of all the capillary arteries in the human body were equal to that of the *aorta*: but ſince this is not the caſe, and the areas of the former greatly exceed that of the latter; the moment of the blood in a capillary red artery, will fall very much ſhort of our computation.

To illuſtrate this; let us ſuppoſe a pipe A of an inch diameter, to be divided into ſeveral branches, and at laſt to terminate in 10000 ſmall tubes *a*, *a*, *a*, *a*, *&c.* each $\frac{1}{100}$ part of an inch in diameter; the ſum of the areas of whoſe tranſverſe ſections is equal to that of A: If

a

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267 grains; but Dr. *Martine* ſeems to have fixed it pretty accurately at 264$\frac{3}{4}$; and, for the ſake of even numbers, I have ſuppoſed it to be 265 grains.

a fluid be puſhed thro' ſuch a ſyſtem of veſſels, with any given force, the velocities in the ſmall tubes *a*,*a*,*a*,*a*, *&c.* will be equal to the velocity in A; and their *momenta* *m*,*m*,*m*,*m*, *&c.* all taken together, will, bating friction, be juſt equal to the *momentum* M in the large trunk A, *i. e.* m 10000 $=$ M or $m = \frac{M}{10000}$. But if another pipe B of the ſame diameter with A be divided ſo as to terminate in 300000 ſmall tubes *b*,*b*,*b*,*b*, *&c.* each $\frac{1}{100}$ part of an inch diameter; then altho' a fluid be puſhed thro' the two trunks A and B with the ſame velocity, and conſequently the *momentum* in them be equal, yet the velocity in any one of the ſmall tubes *a*,*a*,*a*, *&c.* will be to the velocity in any one of the correſponding tubes *b*, *b*, *b*, *&c.* as 30 to 1, and conſequently their *momenta* will be as 900 to 1.

Dr. Keill, having, by meaſuring the arteries of the human body, fixed the proportions of the branches to their trunks, after every diviſion, lays down a method for calculating in what degree the velocity of the blood in the different arteries is affected by the increaſe of the capacity of the veſſels thro' which it flows: * according to this computation, it will be found, that the velocity of the blood in an artery whoſe diameter is $\frac{1}{2000}$ part of an inch, ought to be to its velocity in the *aorta*, as 1 to 345, and conſequently the moment of the blood in ſuch an artery muſt be $345 \times 345 = 119025$ times leſs than we have computed it above, *i, e,* $= \frac{1}{214} \times 119025 = \frac{1}{25471350}$ part of a grain. And ſince a globule of red blood weighs nearly $\frac{1}{30000000}$ part of a grain †, it follows, that the moment or preſſing force of ſuch a globule in its capillary artery, ariſing from the impulſion of the heart, does not exceed twice its own weight.

But even this moment, however ſmall it may appear, muſt be diminiſhed by friction: the preciſe quantity of which, altho' it may, perhaps, be difficult, with any certainty, to determine; yet that it muſt be very conſiderable, will evidently appear from what follows.

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* Keill's Tentamen med. phyſ. 2.

† Medical Eſſays, vol. 2. art. vii. § xi.

1. IF two pipes of equal lengths, whoſe diameters are $\frac{372}{1000}$ and $\frac{90}{1000}$ parts of an inch, be, one after another, ſcrewed into the ſide of a veſſel at the perpendicular diſtance of four feet from the top of the water, and laid parallel to the horizon; the large pipe will diſcharge 179, and the ſmall pipe 6 $\frac{1}{8}$ ounces of water, in half a minute. Hence the velocities of the water in theſe two pipes muſt have been as 1293 and 756; and, were it not for the inequality of the reſiſtance of the air, the velocity in the large pipe would have been ſtill greater, and the velocities in the two pipes pretty nearly as the ſquare-roots of their reſpective diameters *.

HENCE, if we could ſuppoſe a capillary artery, of $\frac{1}{2000}$ part of an inch diameter,

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* Robinſon's animal oeconomy, prop. 1. exp. 2.

meter, to go off directly from the beginning of the *aorta*, without any intermediate branchings; the velocity of the blood in it would be (*cæteris paribus*) to the velocity of the blood in the *aorta*, nearly as $\sqrt{\frac{2}{0.0005}}$ the diameter of the capillary is to $\sqrt{\frac{2}{0.7}}$ the diameter of the *aorta*, *i. e.* as 1 to 37.4; and conſequently the moment of a ſingle globule in ſuch a capillary artery, would be to its moment in the *aorta*, as 1 to 1398.

2. BUT further, the loſs of motion from friction depends not only upon the ſmallneſs of the veſſels, but alſo upon their diſtance from the heart: for, if two cylindrical pipes, whoſe common diameter is $\frac{345}{1000}$ parts of an inch, and whoſe lengths are 2 and 8 feet, be ſcrewed into the ſide of a veſſel full of water, at the diſtance of four feet from the top; the quantities diſcharged in half a minute, will be 97 ½ ounces by the long pipe, and 175 ounces by the ſhort one. Hence the velocities of the

water in the two pipes were as 97½ and 175; ſo that, by the greater quantity of friction in the longeſt pipe, the water loſt above ⅔ of its velocity[*].

3. AGAIN, the velocity of the blood will be different according to the different angles at which the branches go off from their trunks; and the various flexures and convolutions of the ſmall arterial ramifications muſt increaſe the friction in them, and conſequently retard the motion of the blood conſiderably. This ſeems to be confirmed by an experiment of Dr. *Hales*; from which it appears, that the velocity of the blood in the ſmall arteries decreaſes in a greater proportion than it ought to do, by the above mentioned experiments made with ſtreight cylindrical pipes: for, having ſlit up the guts of a dog from one end to the other, on the ſide oppoſite to that where the blood veſſels enter them, and fixed a braſs tube into the deſcending *aorta*, he found that,

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[*] Robinſon's anim. oec. prop. 1. exp. 1.

with a preſſure equal to the force of the heart, only $\frac{1}{3}$ of the water paſſed in a given time thro' the ſlit arteries of the guts that flowed thro' the meſenterics when cut over juſt at their entry into the guts; notwithſtanding that the area of the orifices of all the former exceeded that of the latter, and that the diameters of the cut meſenterics did not exceed four times the diameters of the converging ſlit arteries of the guts.*

FROM what has been ſaid it may appear, that the velocity of the blood will not be the ſame in all the arterys of the ſame diameter, (as ſome have fondly imagined, and been at no ſmall pains to prove), but will be greater or leſs, according to their diſtance from the heart, the exceſs of the areas of the branches above their trunks, the angles at which they go off, and the number and degree of their flexures.

AGREEABLY to this, Dr. *Hales* obſerved, that, in a capillary artery of the lungs of a frog (where the diſtance from the heart is but ſmall, and where the exceſs of the area of all the branches above their trunk, is not near ſo great as in the other parts of the body), the blood moved forty three times faſter than in a capillary artery of one of the muſcles of the lower belly *: and it is probable that, next to the lungs, the blood moves quickeſt thro' the veſſels of the heart. In conſequence of this quick circulation, it muſt be evident, whether we ſuppoſe animal heat to ariſe from the friction of the blood upon the ſides of the veſſels, or from an inteſtine motion among its ſmall particles, that, *cæteris paribus*, more heat muſt be generated in the lungs and heart than any where elſe; and hence the neceſſity of continual ſupplies of freſh air to cool the blood in its paſſage through the pulmonary veſſels. Nor is this opinion founded in theory alone; for, upon trial, it will

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* *Hales*'s ſtatical Eſſays, vol. 2. exp. ix.

appear,

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* Statical Eſſays, vol. 2. p. 68.

appear, that the greateſt heat in any animal is, almoſt always, about the heart. In a jackdaw, the heat below the wing made the mercury in my thermometer riſe to 104 degrees of *Farenheit*'s ſcale; within the *inteſtinum rectum*, it roſe to 107$\frac{1}{2}$; and, when applied to the heart, it reached 109. And, agreeably to this, I have found the heat in a pigeon's heart above a degree greater than within the *inteſtinum rectum*.

Upon the whole, if the moment of a ſingle red globule of blood ariſing from the preſſing force of the heart, does not, in its capillary artery, even bating friction, exceed twice its own weight or $\frac{1}{23471330}$ part of a grain; and if that loſs of motion which it muſt have ſuſtained by friction in its way from the heart thither be conſiderable, as one may reaſonably conclude from what has been advanced upon this head; it will follow, that the real remaining force of ſuch a globule, when it arrives at a red capillary artery, may probably fall ſhort of its own weight, and muſt be ſo extremely ſmall that it can ſcarcely be ſuppoſed ſufficient to overcome the reſiſtance it muſt meet with, in paſſing through a veſſel by which it is cloſely embraced on all ſides, altho' the anterior fluid in the capillary veins were no obſtacle in its way.

I deſire it may be, here, underſtood, that the above calculations are by no means intended as demonſtrations, but rather as illuſtrations, in the preſent argument concerning the force of the blood in the ſmaller veſſels: and, allowing that by them the moment of a red globule in its capillary artery comes out too ſmall, either from our having, with Dr. *Hales*, rated the general force of the left ventricle of the heart too low, or, with Dr. *Keil*, the number of branchings of the arteries, and the proportion they bear to their trunks, too high; yet it muſt be evident, that, in the inferior orders of veſſels, the fluids cannot be propelled by the power of the

heart,

heart, or, which is the ſame thing, that the left ventricle of the heart does not, by its direct projectile force at every contraction, puſh on and move forward the whole circulating fluids in all the veſſels of the body.

Dr. *Hales* obſerved the blood's motion accelerated by every *ſyſtole* of the heart, not only in the ſmall arteries, but alſo in the naſcent capillary veins of the lungs of a frog; and *Lewenhoeck* aſſures us he has ſeen the ſame thing in other parts of various animals: ſo that it is not to be doubted, that the projectile force of the heart reaches at leaſt as far as the capillary arteries of the firſt order, nay, is probably continued, for ſome ſmall way, along their correſponding veins; eſpecially when theſe are not far from the heart.

But that the moment of the blood in the red capillary arteries, at any conſiderable diſtance from the heart, muſt be very ſmall, will appear from an obſervation of Dr. *Hales*; according to which the velocity of the blood in one of theſe arteries in the *abdomen* of a frog, was near 900 times leſs than the equable velocity of this fluid in the *aorta* of a man *; and conſequently 2.6 times leſs than we have computed it to be in a human red capillary: wherefore the exceſs of the moment of a red globule, in ſuch an artery of a frog, above the reſiſtance it had to overcome, only amounted to $\frac{1}{773340600}$ part of a grain, and ſo muſt have fallen a good deal ſhort of $\frac{1}{3}$ of its own weight; ſuppoſing the globules of red blood in a man and a frog to be of the ſame magnitude, which does not ſeem improbable †.

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* Statical Eſſays, vol. 2. p. 69.

If then the remaining moment of a red globule in its capillary artery, after having overcome the reſiſtance of the anterior blood in its correſponding vein, does not amount to $\frac{1}{3}$ of its own weight; it muſt be evident, that the ſerous and ſmaller globules which move along with the red ones, muſt be applied, by the projectile force of the heart, to the orifices of the lateral ſerous arteries with a very inconſiderable force: ſuch a one, ſurely, as will be far from being able to puſh theſe fluids thro' the ſerous, lymphatic, and, for any thing we know, many more inferior orders of veſſels.

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* Statical Eſſays, p. 47. and 68.

† Med. Eſſays, vol. 2. art. vii. § v.

But, to ſet this matter in a ſtill ſtronger light, we ſhall, upon the principles above laid down, endeavour to inveſtigate the force of the heart at the origin of the nerves.

Lewenhoeck tells us, that he diſcovered veſſels in the cortical part of the brain, which could not admit a globule whoſe diameter was $\frac{1}{128000}$ part of an inch *; and he obſerved the fibres of its medullary ſubſtance to be either quadrangular or hexangular: whence he concludes, that they muſt be compoſed of ſmaller fibres, whoſe extreme minuteneſs made it impoſſible for him to diſcover any thing of their figure, nor does he think they can ever be ſeen diſtinctly by human eyes *.

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* De cerebro, p. 35.

Dr. *Porterfield* has, indeed, from an experiment of Dr. *Hook*, computed the diameter of a ſingle nervous fibre to be $\frac{1}{21000}$ part of an inch †: but, as the beſt microſcopes have never been able to diſcover any cavities in the nerves, 'tis certain, that, if they are hollow tubes at all, the diameter of their cavities muſt be a great deal leſs than this, and perhaps fall ſhort of $\frac{1}{200000}$ part of an inch; for a microſcope, which magnifies the diameter of an object 800 times, would, upon this ſuppoſition, make the cavities of the nerves appear equal to a point whoſe diameter is $\frac{1}{250}$ part of an inch, which is an object that may be diſcovered by a good eye. *Lewenhoeck*, 'tis true, towards the end of his days, and when turned of eighty years, pretended oftener than once to have ſeen cavities in the nerves very diſtinctly. But it happens unluckily for this diſcovery, that no body has been able to confirm it ſince his death: nor could he, when alive, tho' he ſaw theſe cavities himſelf, ſhew them to any one elſe; as appears from the following paſſage in his 32d epiſtle: "Id unum "in hoc negotio male me habet, quod "cavitates illas nemini poſſum conſpi"cuas exhibere; nam ſimulac illas ocu"lis meis examinandas admoveo, illico "et minuto citius per exſiccationem "conſidunt." But if the ultimate fibres of the *medulla oblongata* were ſo fine that he could diſcover nothing of their ſhape or figure, as he himſelf confeſſes, it will not be thought probable that he could diſcover the cavities of the nerves, which ſeem to be a production of theſe, and at leaſt equally ſubtile with them.

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* Epiſt. 34.

† Med. Eſſ. vol. iv.

But, leſt any one unaccuſtomed to ſpeculations of this kind ſhould think the motion of a fluid thro' ſuch vaſtly ſubtile veſſels as the nerves almoſt impoſſible, let him reflect a little on the infinite diviſibility of matter, and particularly on the extreme ductility of gold, which may be drawn over ſilver ſo as the thickneſs of the ſkin of gold (in which however the beſt microſcope cannot diſcover the ſmalleſt pore) ſhall not amount to $\frac{1}{12000000}$ part of an inch*; *i. e.* $\frac{1}{60}$ part of what we ſuppoſe the diameter of the cavity of a nerve may be: ſo that the particles of ſuch a leaf of gold ſwimming in a fluid might paſs more eaſily thro' the nerves, than a ſingle globule of red blood does thro' its capillary artery.

FURTHER, a ſoap-bubble, when managed after Sir *Iſaac Newton*'s method, exhibits, juſt before it breaks, a black ſpot upon its ſuperior parts; the thickneſs of which, according to his theory of light and colours, ſcarcely exceeds $\frac{1}{3000000}$ part of an inch. Hence we ſee, that a fluid compoſed of ſoap and water, *i. e.* of alcaline ſalt, lime, oil and water, may be divided, by human art, into parts whoſe diameter is fifteen times leſs than that which we have aſſigned to the nerves, and conſequently that ſuch a compound fluid might eaſily paſs thro' their cavities.

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* Memoires de l'Acad. des Sciences, an. 1713.

LET us then ſuppoſe the diameter of the cavity of a nerve to amount to $\frac{1}{200000}$ part of an inch, and the area of its tranverſe ſection will be 0. 0000000000196; which multiplied into 90 (the height of a column of blood whoſe weight is ſuppoſed equal to the preſſing force of the left ventricle of the heart) gives 0.00000000176 parts of a cubic inch of blood, or $\frac{1}{2140000}$ part of a grain; which would be equal to the moment of the animal ſpirits at the origin of the nerves, ariſing from the impulſive force of the heart, if there were no loſs of motion from friction, and if the area of the tranſverſe ſection of the *aorta* were equal to the areas of the tranſverſe ſections of all the extreme capillary veſſels, in which the numerous branches and ramifications derived from the *aorta*, at laſt terminate. But, if we conſider how greatly the latter muſt exceed the former, and, upon Dr. *Keill's* principles, enter into a computation of the effect which this muſt have upon the motion of the nervous fluid; we ſhall find, that its velocity will be to that of the blood in the *aorta*, nearly as 1 to 20000; and conſequently the moment of the nervous fluid, ariſing from the protruſive force of the heart, will be only equal to $\frac{1}{2140000} \times \frac{1}{4000000000}$ $= \frac{1}{8560000000000000}$.

If we imagine a ſphere to be compoſed of the particles of the nervous fluid, whoſe diameter is equal to the diameter which we have aſſigned to the cavity of a nerve; then, taking its ſpecific gravity to be the ſame with that of water, its weight will amount to $\frac{1}{43228780325614}$ part of a grain, *i. e.* near 19 times more than the force with

which

which it is puſhed forward by the contraction of the left ventricle of the heart, even upon the ſuppoſition that it had met with no reſiſtance from friction in its paſſage through the ſmall veſſels of the brain. Hence the moment of a ſmall ſphere of animal ſpirits in a nerve, is 38 times leſs in proportion to its weight, than the moving force of a globule of red blood in its capillary artery. And the difference of their forces will be ſtill greater, in proportion to the reſiſtance which each has to overcome; ſince the reſiſtance to the motion of a fluid, from friction, muſt be, *cæteris paribus*, as much greater in the nerves than in the red capillary arteries, as the diameter of the latter exceeds the diameter of the former.

But further, ſince, the longer any capillary is, the more will the motion of a fluid be retarded, and conſequently its force be diminiſhed in it; 'tis eaſy to ſee that in the nerves, whoſe cavities are ſo inconceivably ſmall, but whoſe length is generally very conſiderable, the force of the heart, which we have ſhewn to be ſurpriſingly little, muſt be altogether unable to overcome the friction, nay even the mutual attraction of coheſion betwixt them and their fluid, and, conſequently, be of itſelf, and when unaſſiſted by any other power, wholly inſufficient to propell the animal ſpirits to all the different parts of the body. And this, even upon the ſuppoſition that the nerves were continued directly from the extremely minute capillary arteries: but, if we conſider how much the force of the blood muſt be broken in paſſing through the infinitely convoluted and amazingly fine veſſels of the cortical part of the brain, together with the follicles in which theſe are imagined, by ſome, to terminate; what we have been contending for, will appear ſtill more evident.

Lastly, the above reaſoning receives additional weight from thoſe experiments which ſhew that the brain may be nouriſhed, perform its office, and afford ſufficient ſupply of ſpirits for carrying on all the vital and animal functions, altho' the blood is puſhed by the heart into its veſſels with a great deal leſs force than uſual. Thus the learned Dr. *Van Swieten* informs us, that he tied both the carotid arteries of a dog without any obſervable harm to him; on the contrary, he continued twelve days healthful and lively: after which time he opened his ſkull, but could diſcover nothing praeternatural in the brain [*]. Now, as in this dog the brain could only be ſupplied by the vertebral arteries which inoſculate with the carotids, the velocity, and conſequently the moment of the blood, muſt, at the ſame time that it was conſiderably leſſened in the ramifications of the former, have been ſo remarkably diminiſhed in thoſe of the latter, by reaſon of the ſmallneſs of the branches with which they communicate, compared with the trunks of the carotids, as to ſhew, beyond doubt, that the ſecretion of the nervous fluid, and its derivation to the ſeveral parts of the body, do not depend ſo much upon the force of the heart as has been generally imagined, but muſt be, in a great meaſure, owing to ſome other cauſe.

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[*] Comment. in Boerh. aphor. vol. 1. p. 266.

Having ſhewn how inconſiderable the moment of the fluids ariſing from the projectile force of the heart muſt be, in the inferior orders of veſſels, and particularly at the origin of the nerves; we come now to take a view, ſomewhat different, of the matter, and to compare the real force of the left ventricle of the heart with the obſtacles it has to overcome, upon the ſuppoſition that at each *ſyſtole* it puſhes forward the whole circulating fluids in all the arteries and veins of the body.

Borelli computed the reſiſtance, which the blood meets with in circulating thro' all the veſſels of the human body, to be equal to 180000 pounds weight [*]: but, tho' this be over-rating the matter very much, yet, after all the abatements that can be reaſonably allowed, there will remain a reſiſtance by much too great to be overcome by the force alone of the left ventricle of the heart; a force, which cannot, in man, amount to 60 pounds weight [†]; as far as can be gathered from the lateſt and beſt experiments, which have been made on other animals, in order to determine the preſſing power of their heart. Yet, inconſiderable as this force is, it is not to be regarded as that communicated to the blood in the *aorta*, but only as the preſſure or weight ſuſtained by the whole internal ſurface of the left ventricle of the heart juſt when it begins to contract; and the force with which the blood is impelled into the *aorta*, will (ſince fluids preſs equally *undequaque*) bear no greater proportion to this, than the *area* of the orifice of the *aorta* does to the whole internal ſurface of the left ventricle of the heart; *i. e.* ſuppoſing the area of the orifice of the *aorta*=0.5 of a ſquare inch and the internal ſurface of the left ventricle=15 ſquare inches*, as 1 to 30; and therefore the force with which the blood is puſhed into the *aorta*, muſt fall ſhort of $\frac{1}{30}$ of 60 pounds weight. Hence a reſiſtance in the *aorta*, equal to two pounds, will require a force of above 60 pounds exerted by the whole internal ſurface of the left ventricle of the heart to overcome it: from which it follows, either that the reſiſtance to the motion of the blood in the *aorta* and all its branches and ramifications muſt be leſs than two pounds, which I believe no body will affirm; or elſe that the protruſive force of the left ventricle of the heart alone, is unable to drive the blood thro' all theſe veſſels, and conſequently inſufficient, without the aſſiſtance of ſome other power, to carry on the circulation.

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[*] De motu animal. part. 2. prop. 73.

[†] Dr. *Hales* makes it only 51 pounds, *Statical Eſſays*, vol. 2. p. 40.

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* *Hales* loc. citat.

If any one ſhould, on this occaſion, have recourſe, with the learned *Borelli*, to the *vis percuſſionis*, we need only obſerve that the force of the heart, is evidently not a percuſſive but a preſſing one; ſo that, altho' the leaſt percuſſive force may be greater than any finite quieſcent reſiſtance, yet this will not hold true of a preſſing force, which, in order to have any ſenſible effect, muſt be greater than the reſiſtance it has to overcome: to ſay otherwiſe, is to affirm that, with the preſſing force of one's hand, the greateſt mountain might be moved out of its place.

Nor is Dr. *Keill's* account of this matter more ſatisfactory, *viz.* that, the blood being once put in motion, a very ſmall force in the heart may be ſufficient to keep it always in this ſtate: for this force muſt be equal to the loſs of motion, ſuſtained by the blood, in every circulation, and conſequently to the reſiſtance which this fluid meets with in its paſſage thro' all the veſſels of the human body; a reſiſtance by far too great to be balanced by the few ounces to which the *Doctor* has reduced the force of the left ventricle of the heart *.

But that the foundation upon which Dr. *Keill* proceeds is falſe, and that the heart can really communicate a new motion to the blood when the old one is, in a great meaſure, loſt, and after all the fluids have been for ſometime almoſt intirely at a ſtand, is evident from the recovery of people who have lain for ſometime in a *ſyncope*, and from the revival of the ſleeping animals, which are, in appearance, dead all the winter-ſeaſon. But further, ſince the blood, when it returns to the right ventricle of the heart, has ſcarce $\frac{1}{10}$ of the force with which it was thrown into the *aorta* *, 'tis plain that it acquires, every circulation, $\frac{9}{10}$ of its force in paſſing thro' the heart and lungs.

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* Tentam. med. phyſ. 3. de vi cordis.

THUS much being ſaid to ſhew that the force of the heart is, of itſelf, not ſufficient to carry on the circulation, we ſhall next briefly conſider the alternate contraction of the *aorta* and its branches, which has been juſtly reckoned among the chief cauſes of the motion of the blood.

THE blood thrown out at every *ſyſtole* by the left ventricle of the heart, is not inſtantly tranſmitted thro' the capillary arteries into their correſponding veins, but the greateſt part of it is accumulated in the now-dilated arteries, and is, during their ſucceeding contraction, conveyed on thro' the ſmaller veſſels. This contraction however of the arteries may, perhaps, be conſidered, rather as a continuation of the heart's force, than as any new power impreſſed on or communicated to the blood; ſince it does not appear that the arteries contract with a greater force than that by which they were dilated. But, whatever may be the force with which the *aorta* and its branches reſtore themſelves, we know certainly that it is leſs than the ſyſtolic power of the left ventricle of the heart; becauſe the blood is obſerved always to be projected to a greater diſtance from a cut artery during its *diaſtole*, than in the time of its *ſyſtole*. Whence it follows that, if the force of the heart is inſufficient to account for the motion of the fluids thro' the inferior orders of veſſels, the alternate contraction of the muſcular coat of the *aorta* and its branches muſt be ſo likewiſe. It is, however, to be obſerved that the ſanguiferous arteries, whoſe numerous branches are diſperſed every where thro' the body, muſt not only, by their alternate contraction, contribute to puſh forward their contained fluids, but alſo, by their dilatation, ſo compreſs the inferior orders of veſſels, as ſomewhat to promote the motion of the fluids in them *. I ſhall only add on this head, that, as the alternate contraction of the arteries depends intirely upon their preceeding dilatation by the heart, ſo, in the ſerous and inferior orders of arterial veſſels to which the projectile force of the heart ſeems not to reach, there is no ſuch alternate dilatation and contraction to be obſerved †.

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* *Hales*'s Statical Eſſays, vol. 2.

WITH reſpect to gravity, which ſome have reckoned among the cauſes promoting the circulation, it is ſufficient to obſerve, that in a horizontal poſition of the body, it can have no effect; and, in an erect one, it muſt retard the return of the blood by the *vena cava inferior*, as much as it promotes its motion downwards in the *aorta* and its branches.

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* Vid. Medical Eſſays, vol. 5. p. 2 Edit. 3. page 39.

† *Lewenboeck* epiſt. 65. pag. 167.

There is ſcarcely any thing that will ſooner or more naturally ſtrike the mind of one who inquires into the cauſes of the motion of the fluids in the very minute veſſels of animals, as well as vegetables, than that ſurpriſing power of attracting liquors which capillary tubes are endowed with. But altho' the attractive power of capillary tubes may aſſiſt us in accounting for the imbibition of fluids by the veſſels commonly called abſorbents, as we ſhall afterwards have occaſion to ſhew; yet it muſt appear evident to every one acquainted with the *phænomena* of theſe tubes, that this attraction can be of no uſe in promoting the circulation of the blood in the capillary arteries and veins: ſince theſe veſſels are always full; or, if they were not, the fluids would be determined by it, equally backward towards the larger arteries as onwards to the veins.

SECT. II.

That the vibratory motion of the ſmall veſſels of animals is the principal cauſe promoting the circulation of the fluids.

HAVING ſhewn the inſufficiency of the powers already mentioned to account for the circulation of the fluids in the very ſmall veſſels of animals, we ſhall now proceed to explain what we imagine to be the principal cauſe of this circulation.

ALTHO', as has been obſerved above, the regular alternate pulſation of the arteries does not extend beyond the capillaries of the firſt order, except, perhaps, in places very near the heart; yet we are not to conſider the ſerous, lymphatic, and other ſtill ſmaller veſſels, as unactive canals no ways contributing to promote the circulation of their different fluids: on the contrary, it ſeems highly probable, that theſe veſſels are continually agitated with very ſmall alternate contractions, to which the circulation in them is, in a great meaſure, owing.

MANY phyſiological writers have ſuppoſed an oſcillatory motion in the ſmall veſſels of animals*, but few have ſaid any thing ſatisfactory concerning the cauſe of this motion. *Baglivi* ſuppoſed the membranous parts of the body to derive their oſcillations from the *dura mater*; and the vaſcular ſyſtem and fleſhy fibres, theirs from the head: but, as it is now paſt doubt that the *dura mater* has no other motion than what ariſes from the pulſation of its own veſſels or thoſe of the brain; and as the alternate contraction of the arteries depending upon their preceeding dilata-

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* Among others, the learned Dr. *De Gorter*, in his treatiſe *de motu vitali*, has not only admitted a vital oſcillatory motion in the ſmall veſſels, but endeavours to ſhew that, without this, the force of the heart would be unable to carry on the circulation, § lvi. *&c.*

tion by the blood thrown out by the heart, has no place in the ſerous, lymphatic, and inferior orders of veſſels; the vibratory motions of theſe canals muſt be deduced from ſome other cauſe.

Many experiments and obſervations ſhew that the muſcular fibres of animals are ſo framed, as to be readily excited into contraction by a *ſtimulus*. The ſmall veſſels, therefore, which are endowed with a muſcular coat, as well as the larger ones, muſt neceſſarily be agitated with alternate contractions, as often as they are acted upon by any thing capable of gently irritating them; but ſuch are the blood and finer fluids derived from it, which, while they ſlowly glide through the ſmall veſſels, ſtimulate their internal ſurface, ſo as to excite them into gentle but continually repeated contractions.

Some of the greateſt philoſophers and phyſicians, of antient as well as later times, have imagined the blood to be a very active fluid, endowed with uncommon qualities, and, as it were, the fountain and ſource of life in animals[*]: nor do they ſeem to have been led into this opinion ſo much from any favourite theory, as from experiments and obſervations made on living and dying animals. But, without entering into, much leſs defending, the peculiar notions of theſe authors concerning the blood, we ſhall only ſay, that this fluid is extremely well fitted to act as a gentle *ſtimulus* upon the ſenſible fibres of animals, whether we conſider its compoſition, heat, or inteſtine motion: for, while the ſaline and other acrid particles in the blood render it fit to irritate the tender veſſels, its heat and inteſtine motion keep all its parts in a perpetually vibrating ſtate, which muſt increaſe their ſtimulating power[†]. Agreeably to this, we find, that, in many

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[*] *Ariſtot.* hiſtor. animal. lib. 3. cap. 19; et *Harvey* de generatione animal. exercitat. li. lii. et lxxi.

[†] See an Eſſay on the vital and other involuntary motions of animals, ſect. 3.

ny inſects and ſome larger animals, the circulation becomes more languid, as the weather grows colder, and, in the winter ſeaſon, is altogether at a ſtand, till, by the heat of the returning ſpring, the particles of the fluids begin to be briſkly agitated, and conſequently the ſolids ſtimulated into contraction. Doctor *Harvey* has long ſince remarked, that the hearts of ſeveral ſhell-fiſhes are only ſeen to beat in warm weather †; and the curious obſervations of *Reaumur* have ſhewn us, that the lives of inſects may be lengthened or ſhortened, and made more or leſs active, by expoſing them to different degrees of heat and cold ‡.

THUS much being ſaid to ſhew, that the blood is well fitted to act as a *ſtimulus*, we ſhall offer ſome further conſiderations to prove, that the ſmall veſſels are, by its influence, really excited into alternate contractions. And,

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† De motu ſang. cap. xvii.

‡ Hiſtoire des inſectes, tome 2. memoire 1.

1. We are led to conclude this from what we obſerve in the larger canals and veſſels of animals. Thus the ſeveral portions of the inteſtinal tube are ſolicited into alternate contractions by the aliment, air, and bile, ſtretching their coats and ſtimulating their internal ſurface: and, as we imagine an alternate motion in the ſmall veſſels neceſſary to promote the circulation of the fluids in them, ſo we know certainly, that the periſtaltic motion of the guts is the principal cauſe which conveys the digeſted aliment down towards the *anus*.

Not only the auricles and ventricles of the heart, but alſo the trunks of the *venae cavae* adjoining to the right *ſinus venoſus*, are continually agitated with alternate contractions*. The trunks of the *venae cavae* preſerve this motion, in animals newly dead, a conſiderable time after the pulſation of the heart has ceaſed; but no ſooner is the blood contained in theſe veſſels evacuated, and all new ſupplies intercepted by ligatures, than their ſides collapſe, and remain without the ſmalleſt motion*: whence we are led to conclude, that the alternate contractions of theſe veins are, like thoſe of the heart, owing to the blood acting upon them as a *ſtimulus*.

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* Eſſay on vital motions, &c. p. 97 and 354.

It is generally allowed by phyſiologiſts, that the *ſyſtole* of the larger ſanguiferous arteries, in which a remarkable pulſation obtains, is owing, not only to their elaſticity, whereby they endeavour ſimply to recover themſelves, but partly alſo to a proper muſcular contraction of their tendineo-carnous coat: and, as this is excited by the blood puſhed into them by the heart, which, at the ſame time that it diſtracts their fibres, gently irritates their internal ſurface; it ſeems highly reaſonable to allow, that the ſmaller veſſels, endowed at leaſt with equal ſenſibility, muſt be excited into feeble but continually repeated contractions, by the gentle *ſtimulus* of their circulating fluids.

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* Bartholin. epiſt. cent. iv. p. 109, *&c.*

FURTHER, as there are ſome of the more imperfect animals which have no heart, the circulation in them muſt be owing to the contractile power of the veſſels themſelves excited into action by the *ſtimulus* of the fluids. And that the veſſels of thoſe animals which, in a natural ſtate, have a heart, are endowed with a ſimilar power, ſeems proved by examples of monſters wanting a heart or any thing analogous to it*, in whom the fluids muſt have circulated chiefly by the power of the veſſels.

2. A variety of facts might be mentioned, which clearly demonſtrate an alternate contractile power in the ſmall veſſels of animals, and that this is exerted more or leſs according to the degree of irritation affecting them.

a THUS,

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* Van Swieten comment. in Boerhaave aph. vol. 1. p. 256; and Hiſtoire de l'acad. des ſciences, 1703; & Memoires, 1740.

α Thus, the ſteams of warm ſpirit of wine received into the eye, not only cauſe a greater flow of tears from their veſſels, but, in a few ſeconds, produce an artificial inflammation in them, that is, they make the globules of red blood enter the ſerous or lymphatic veſſels of the *conjunctiva*. Now, as this additional moment of the blood, whereby it is enabled to dilate theſe veſſels, cannot proceed from the heart or larger arteries, ſince their force is not, nor can be altered in the preſent caſe; it muſt be owing to the extraordinary oſcillatory motion excited in the veſſels of the eye by the ſteams of the ſpirit of wine.

I preſume it will not be alledged, that the vapour of ſpirit of wine raiſes an inflammation in the eye, by conſtringing its veſſels ſo as to occaſion an obſtruction in them, and that this obſtruction afterwards produces the inflammation, by leſſening the number of veſſels thro' which the blood paſſes, and conſequently increaſing its force upon the obſtructed ones: for, not to inſiſt on what might be eaſily proved, that no obſtruction can ever produce an inflammation except in ſo far as it gives riſe to an unuſual irritation; the ſpirit of wine ſhould, by conſtringing the ſerous and lymphatic veſſels of the *conjunctiva*, enable them to ſuſtain this additional force.

But further, why does tepid milk and water, or a poultice of bread and milk, leſſen an inflammation of the eye, while acrid aſtringent and ſpirituous things increaſe it? According to the doctrine of inflammation from mere obſtruction together with an increaſed force of the heart and larger arteries, one would think that the former ſhould, by relaxing the ſmall veſſels, expoſe them to be ſtill more and more dilated by the increaſed force of the blood, and ſo increaſe the inflammation; while the latter ſhould, by conſtringing thoſe veſſels, enable them not only to reſiſt the blood impelled by the heart, but alſo expell the obſtructing red globules. But the truth of the matter is, that the tepid milk and water and poultice, by relaxing the veſſels, leſſen or remove the irritation and ſenſe of pain, which, by raiſing uncommon contractions in the ſmall veſſels, was the cauſe of the inflammation; while acrid aſtringent and ſpirituous applications, tho' they tend to conſtringe the veſſels, yet, by increaſing their vibratory contractions, greatly augment the motion of the blood in them, and therefore muſt neceſſarily increaſe the inflammation.

β The heat, redneſs and inflammation, brought on the ſkin by bliſters and ſinapiſms, are not owing to any increaſe of the heart's force, or of the moment of the blood in the larger veſſels, tho' this is often an effect of their application; but merely to the action of thoſe irritating ſubſtances on the cutaneous veſſels, whereby the motion of the fluids in them is greatly augmented.

γ THE ſudden redneſs and glowing warmth of the face, which, in the fair ſex eſpecially, accompanies a conſciouſneſs of ſhame, and is commonly diſtinguiſhed by the name of *bluſhing*, can only be ſatisfactorily accounted for, from an increaſed oſcillatory motion of the ſmall veſſels of the face*.

δ THE extraordinary flow of ſpittle which happens to hungry perſons from the ſight or even the remembrance of grateful food, and the profuſe ſecretion of urine which hyſterical people are frequently ſubject to, cannot be explained without having recourſe to an increaſed motion ſuddenly excited in the ſmall veſſels of the ſalivary glands and kidneys; and clearly ſhew that the quantity of ſpittle and urine ſeparated by theſe organs, does not depend ſo much upon the force with which the

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* See an Eſſay on the vital and other involuntary motions, p. 101. and 102.

blood is determined into their veſſels by the heart, as upon the greater or leſſer vibratory motions of the ſecerning veſſels themſelves. And in the ſame manner, is it not reaſonable to believe, that the motion of the fluids in the ſmalleſt veſſels every where thro' the body, is as much, perhaps more, owing to their gentle alternate contractions, than to the force of the heart and larger arteries?

THE ſecretion of tears, which is very little affected by the different forces with which the blood is impelled by the heart, is immediately increaſed in a very great degree by acrid applications to the eyes, or by certain paſſions of the mind.

IN the firſt caſe, the greater ſecretion is owing to the acrid matter, which, by its irritation, raiſes an uncommon vibratory motion in the lachrymal veſſels. Nor can it be, with reaſon, objected here, that acrid things, applied to the eyes or received into the mouth, occaſion a greater flow of tears or ſpittle, not by raiſing any ſtronger oſcillatory motion in the veſſels of the lachrymal and ſalivary glands, but merely by conſtringing their excretory ducts, and ſo ſqueezing out the liquors contained in them; ſince the quantity of tears and ſpittle diſcharged in ſuch caſes ſhews, that not only the excretion but ſecretion in theſe glands is greatly increaſed. And if an irritation of the *pelvis* of the kidney, or *ureter*, from a ſtone lodged there, often occaſions an uneaſy ſenſation in the extremity of the *urethra* *; is it not reaſonable to think, that, upon the application of ſtimulating things to the orifices of the lachrymal and ſalivary ducts, theſe will not be affected alone, but the irritation will, in ſome degree, be communicated to the ſmall ſecerning veſſels of their reſpective glands, ſo as to excite in them ſtronger and more frequently re-

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* Van Swieten Comment. in Boerh. Aphor. vol. i. p. 301. et Morton de Phthiſi, lib. ii. c. 3.

peated contractions, and conſequently increaſe their ſecretions?

THE flow of tears which accompanies certain affections of the mind, is, like the greater ſecretion of ſpittle from the ſight of grateful food, and the heat and redneſs of the face from a conſciouſneſs of ſhame, owing to an unuſual vibratory motion excited in the lachrymal veſſels in conſequence of theſe affections, and not to any compreſſion which the lachrymal gland may ſuffer from ſome of the neighbouring muſcles which are then brought into contraction; for no degree of alternate compreſſion applied to this gland remarkably increaſes the ſecretion of tears, unleſs its veſſels, or thoſe of the eye, are thereby irritated.

3. WE have already ſeen, that an increaſed oſcillatory motion in the ſmall veſſels occaſions a quicker flow of liquors thro' them: and the following ſhort hiſtory will ſhew, that, when this motion is much diminiſhed or wholly

ſuſpended, theſe veſſels collapſe, and the circulation in them either becomes very languid, or ceaſes altogether.

A boy betwixt four and five years of age was, on *Saturday*'s afternoon, ſuddenly ſeized with an apoplexy or abolition of ſenſe and voluntary motion. On *Sunday*'s morning, at nine o' clock, when I firſt ſaw him, his pulſe was full and quick, and his eyes had ſomething of a glazed look; but in the evening this was more remarkable. *Monday* a little before noon, he was ſtill alive, but his breathing was very laborious and his pulſe ſmall and quick; at this time, his eyes were more ſhrivelled, than they uſe to be in thoſe who have been ſeveral hours dead.

THIS glazed appearance of the eyes could not be owing to the diminution of the heart's force, ſince the pulſe was full and ſtrong for twenty four hours after the diſeaſe came on: nor can the failure of the pulſe, afterwards, account for their ſhrivelling more than is uſual in perſons newly dead. But if the circulation of the fluids in the ſmall veſſels be chiefly owing to a vibratory motion in them, and if this muſt ceaſe when the influence of the nerves is intercepted; in this boy, whoſe brain, eſpecially its anterior part, was ſo remarkably obſtructed, the motion of the fluids in the very ſmall veſſels of the *cornea* and the ſecretion of the aqueous humour muſt have been greatly diminiſhed, and hence the dimneſs and ſhrivelling of the eyes.

THE withering of a member that is palſied, or deprived of the nervous power, is to be accounted for in the ſame manner; and is a proof that the circulation of the fluids thro' the inferior orders of veſſels, is not ſo much owing to the force of the heart, as to the action of theſe veſſels themſelves. This withering of a palſied member has made ſome imagine, that nutrition is performed by the nerves: but the *phænomenon*, we ſee, is eaſily accounted for without this ſuppoſition; and there are good reaſons to think that the nerves are *ſolely* ſubſervient to motion and ſenſation.

4. LASTLY, altho' the alternate contractions of the ſmaller veſſels, which we have been contending for, are not remarkable enough to be diſcerned in moſt animals; yet they may be clearly ſeen in the legs of a bug: in the ſmall veſſels of which, an extraordinary vibration is diſcovered by the microſcope *.

THE objection againſt the reality of a vibratory motion in the ſmall veſſels of animals, becauſe the microſcope ſhews no ſuch thing in moſt animals, is of no great weight; ſince it cannot be doubted, that the particles of all bodies, eſpecially fluids, are affected by heat with a perpetual oſcillatory motion; and yet, unleſs the heat be great, the eye, even aſſiſted by the beſt microſcopes,

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* Baker on the microſcope, p. 130.

croſcopes, cannot diſcern any ſuch thing.

FURTHER, ſince the microſcope only ſhews the circulation of the fluids in the red capillary arteries, but not in the ſerous, lymphatic, and many inferior orders of veſſels, can it be expected that any alternate vibratory motion ſhould be diſcovered in theſe veſſels? Or, is it reaſonable to deny an alternate motion to all veſſels or particles of matter which are too ſmall to fall under the notice of our ſenſes?

ALTHO' the branches of the vine were tranſparent, ſo that the motion of the ſap in its veſſels could be ſeen by the help of a good microſcope; yet it is very probable we ſhould not be able to diſcover any vibratory motion in them: and yet the force of the ſap in the bleeding ſeaſon ſhews, that, beſides attraction, there muſt be a real propelling power exerciſed by the veſſels of the vine*.

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* Vid. Hales's Statical Eſſays, vol. i.

If the diameter of the *aorta* in its *diaſtole* does not exceed its diameter when contracted above $\frac{1}{5}$ of a line, *i. e.* $\frac{1}{50}$ of its diameter*; and if the change of diameter, which happens in the red capillary arteries and inferior orders of veſſels from their vibratory contractions, be three times leſs in proportion to the magnitude of theſe veſſels than the difference of diameter in the *aorta*, ariſing from its alternate *diaſtole* and *ſyſtole*; then the difference between the greateſt and leaſt diameter of a capillary artery capable of receiving only one globule of red blood, when moſt dilated or contracted, will be equal to $\frac{1}{150}$ part of its diameter: *i. e.* ſuppoſing its diameter $\frac{1}{2000}$ of an inch, $=\frac{1}{300000}$ of an inch; and the ſpace deſcribed by each ſide of ſuch an artery, when it performs one of its ſmall vibratory contractions, will be only equal to $\frac{1}{600000}$ of an inch, which is greatly too ſmall to be diſcerned by the beſt microſcope.

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* Vid. Weitbrecht in Comment. Academ. Petropolitan. vol. vii. p. 314.

HAVING thus endeavoured, by a variety of arguments, to ſhew, that the ſmall veſſels of animals are, thro' the gentle *ſtimulus* of the fluids, continually agitated with alternate contractions; we ſhall now, briefly, point out their uſe in carrying on the circulation. And it muſt appear evident to every one, that the inferior orders of veſſels will not only not retard the motion of the fluids, but greatly promote it; ſince every ringlet of them will, like a little heart, by its alternate contractions, puſh on its contained fluid. Nor ought theſe contractions, however weak and imperceptible, to be thought unable to produce this effect; ſince the motion of the fluids in the very ſmall veſſels is far from being rapid, and juſt ſuch as might be expected to ariſe from this cauſe. Dr. *Hales* has obſerved, that, in a capillary red artery in one of the muſcles of the *abdomen* of a frog, the blood moved only an inch in a minute and a half[*]: and it is probable, that, in the fineſt ſecretory veſſels of the brain, the fluids may not move above a Pariſian line or $\frac{1}{12}$ of an inch in a minute, *i. e.* not twice as faſt as the *minute* hand of a ſmall-ſized watch.

If it be objected, that, as the capillary arteries and veins are deſtitute of valves, their alternate contractions muſt puſh the fluids equally back towards the heart, as onwards to the larger veins: it may be ſufficient to anſwer, that, the reſiſtance ariſing from the ſemilunar valves of the *aorta*, and from the force of the heart and larger arteries *a tergo*, being greater than that which oppoſes the tranſmiſſion of the fluids into the larger veins; the fluids acted upon by the ſmall vibrating veſſels, muſt neceſſarily be determined towards the latter. But further, why may not the alternate contractions of the ſmall veſſels, like the periſtaltic motion of the guts, proceed in ſuch manner, as to impell their fluids more remarkably onwards to the veins than backwards to the larger arteries?

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[*] Statical Eſſays, vol. ii. p. 68.

Upon the whole, as we conceive the motion of the blood in the larger veſſels, and even capillaries of the firſt order, to be owing to the alternate *ſyſtole* of the heart and arteries; ſo in the ſerous, lymphatic, and ſtill ſmaller veſſels, where this force either reaches not at all, or is greatly diminiſhed, the circulation ſeems to be carried on, chiefly by the vibratory motions of theſe veſſels themſelves: and, the finer fluids being, in this manner, tranſmitted into the larger veins, the pulſation of neighbouring arteries, action of voluntary muſcles, and alternate compreſſion made upon all the contents of the *abdomen* and *thorax* by the motion of reſpiration, will promote their return to the heart along with the red blood in the *venae cavae*.

WHAT we have ſaid of the circulation of the fluids in general, we would have underſtood alſo of their motion in the ſecretory pipes of the ſeveral glands. In thoſe glands whoſe veſſels are moſt patulous, the ſecretion may be partly, and indeed in a good meaſure, carried on by the force of the heart and larger arteries; a proof of which ſeems to be the bloody urine paſſed by ſuch as have weak kidneys, after violent exerciſe: but in other glands, whoſe ſtructure is finer, and particularly in the brain, the motion of the fluids in the ſecretory and excretory veſſels ſeems to be much leſs owing to the force of the arterial blood *a tergo*, than to the gentle vibratory contractions of the veſſels themſelves.

WITH regard to the nerves, which are generally conſidered as the excretory ducts of the brain; it is probable, that the derivation of their fluid to the various parts of the body is not only owing to a gentle oſcillatory motion in them and their ſurrounding membranes, but alſo, in ſome degree, to their attraction as capillary tubes; for no ſooner can there be a waſte of this fluid at the extremity of any nerve, whether this happens from exhalation, alternate compreſſion of the neighbouring parts, or any other cauſe, than, by its attractive power, it will be filled again. In the other glands, however, whoſe excretory ducts, by their union, ſoon form pretty large canals, no ſuch attraction will have place.

1. FROM what has been ſaid, it may appear, that we are not to conſider the force of the heart and contraction of the larger arteries, as the ſole cauſes of the circulation of the fluids in animals. The whole vaſcular ſyſtem is endowed with a moving power, which is conſtantly excited into action by the *ſtimulus* of the circulating fluids; ſo that while the ſmall veſſels, by means of friction, deſtroy in part the moment of the juices, they, at the ſame time,

communicate, by their gentle vibratory contractions, a new impulſe to them. Every part therefore of the vaſcular ſyſtem, as well as the heart and larger arteries, nay every ringlet even of the ſmalleſt veſſel, is to be conceived as promoting the circulation of the fluids; that great work, upon which the life of the whole depends, and, in carrying on which, every part almoſt of the body is active.

2. If the motion of the fluids in the inferior orders of veſſels be not ſo much owing to the force of the heart and larger arteries, as to the gentle alternate contractions of thoſe veſſels themſelves, we may eaſily ſee, why frictions, warm, penetrating, and ſtimulating fomentations, and cataplaſms, &c. are often more ſucceſsful, than internal medicines, in removing obſtructions in the ſerous, lymphatic, and other ſmall veſſels; ſince they not only contribute to attenuate the obſtructing matter,

but greatly increaſe the oſcillatory motion of theſe veſſels. For the ſame reaſon it is, that the warm mineral waters, pump'd with conſiderable force, upon a part affected with the rheumatiſm or *ſciatica*, have effected a cure after other remedies had been uſed in vain.

WARM ſpirit of wine, either alone or mixed with other things, proves often a good deobſtruent: yet I have known ſome people who were afraid to uſe it with this intention, becauſe it is known to coagulate the *ſerum* of the blood: but their fears were without foundation; for the quantity of ſpirit of wine, which enters by the pores of the ſkin, is ſo ſmall as to be in no danger of producing any *coagulum*; beſides, as it is taken in by the abſorbent veins, it muſt go to the heart and be mixed with the maſs of blood, before it can come at the obſtructed veſſels. But, altho' little is to be expected from the reſolving, and nothing is to be dreaded from the coagulating power of the ſpirit of wine, yet it proves, in many caſes, a good deobſtruent, by raiſing an uncommon vibratory motion and heat in the veſſels of the part to which it is applied.

3. If the circulation in the ſmall veſſels be, in a great meaſure, owing to their oſcillatory motion excited by the *ſtimulus* of the circulating fluids, it will follow that, when theſe veſſels, in any part of the body, are affected with an extraordinary irritation, they muſt neceſſarily be agitated with much ſtronger and more frequently repeated contractions than uſual: whence the force of the blood in them will be greatly increaſed; in conſequence of which the part will be inflated, and globules of red blood will be forced into the ſerous veſſels, *i. e.* an inflammation will be produced; and this muſt happen, whether the force of the blood be, or be not, increaſed in the other veſſels of the

body. An inflammation, therefore, is not owing to an increaſed force of the heart and larger arteries conſequent upon an obſtruction, as ſome authors of great name have imagined; but to an increaſed oſcillatory motion in the ſmall veſſels, whether this ariſes from ſome obſtructing matter diſtracting their fibres, or acrid matter irritating them. An obſtruction without an irritation in the obſtructed part, never occaſions an inflammation; but the irritation of any ſenſible part with a ſharp inſtrument, or acrid matter, never fails to produce this effect, altho' there be no preceeding obſtruction, nor increaſe of the heart's force. When a large artery is tied in the operation of the aneuriſm, we don't find, that the increaſed moment of the blood in the neighbouring arteries, produces an inflammation in the arm; but, when a tendon is wounded in blood-letting, or a little acrid matter is collected below the nail, a remarkable pain, ſwelling and inflammation of this member follow. However, altho' an increaſed force of the blood in the large veſſels is not the cauſe of an inflammation, yet it is frequently the conſequence of it: for, as often as the inflammation is large, or the part inflamed very ſenſible, the whole nervous ſyſtem will be ſo affected by the pain, as to render the heart and larger arteries more irritable, at the ſame time that the blood, now vitiated by the obſtruction and inflammation, muſt act upon them as a ſtronger *ſtimulus* than uſual. Hence we may ſee, why, in inflammations, the pulſe is often little changed till the diſeaſe has continued for ſome conſiderable time. In inflammations of the ſtomach, guts, and *uterus*, the pulſe, tho' much quickned, often continues ſmall; becauſe, on account of the particular ſympathy between their nerves and thoſe of the heart, this muſcle is rendered ſo irritable, as to contract before its ventricles are filled with the returning venous blood.

FROM what has been ſaid it may appear, that, in the cure of inflammations, beſides diminiſhing the force of the circulation in general by blood-letting, a particular regard is to be had to the veſſels of the part affected, whoſe extraordinary contraction ſhould be leſſened by proper emollient and anodyne applications, and, in many caſes, by bliſtering the neighbouring parts. My ingenious friend Dr. *Pringle* has often obſerved the good effects of bliſters, even when early applied, in pleuriſies and other internal inflammations *. And I have ſeen a bliſter, in twelve or fourteen hours, leſſen, remarkably, the frequency of the pulſe in an *angina*, after blooding once and again had done little this way. I know many phyſicians have entertained prejudices againſt bliſtering in inflammations, becauſe,

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* See his obſervations on the diſeaſes of the army, 1 Edit. p. 173, 178, and 179.

cauſe, by their irritation, they increaſe the force of the circulation in general: but, not to mention the good effects they may have by attenuating the obſtructing matter, and making a conſiderable derivation of ſerous humours from veſſels which are nearly connected with thoſe of the part affected; if the account we have given of inflammations be true, it muſt follow, that, altho' the *material* cauſe of an inflammation, *i. e.* the acrid or obſtructing matter, be not immediately removed by bliſtering; yet, if, according to *Hippocrates*'s obſervation *, the painful ſenſation in the inflamed veſſels be leſſened by its means, the extraordinary oſcillatory motions of theſe veſſels, and conſequently the cauſe continuing and increaſing the inflammation, muſt be alſo leſſened. Hence it appears, that a bliſter,

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* Δυο πόνων ἅμα γινομενων, μὴ κατα τ̃ ἀυτὸν τοπον, ὁ σφοδρατερως ἀμαυροῖ τ̃ ἕτερον. Duobus doloribus ſimul obortis, non in eodem loco, vehementior obſcurat alterum. Aphor. lib. 2. No. 46.

ſter, tho' it tends to increaſe the force of the circulation in general, may yet leſſen the *impetus* of the blood upon the veſſels of an inflamed part more remarkably, than even blood-letting itſelf.

WHAT has been ſaid of bliſtering, may be applied alſo to cupping and ſcarifying in pleuriſies, *angina's*, *&c.*

SINAPISMS, laid to the ſoles of the feet, remove or leſſen ravings, not by determining the blood more copiouſly to the inferior extremities, for their effect in this reſpect is altogether trifling; but by raiſing a very conſiderable pain, which ſo affects the mind as to render it leſs ſenſible of the unuſual *ſtimulus*, or irritation in the brain, or its membranes, *i. e.* of the cauſe producing and continuing the *delirium*. Nor is it material to what part of the body thoſe cataplaſms are applied; for a ſtrong *delirium*, in a fever, has been removed by the application of a ſinapiſm, by miſtake,

take, inſtead of a poultice of theriac, to the region of the ſtomach.

We may alſo, from what has been ſaid, ſee how ravings, phrenſies, and madneſs have been cured by the power of muſic [*], or by a ſudden fright [†]; for theſe, by greatly affecting the mind and fixing its attention, not only render it leſs ſenſible of the diſordered ſtate of the brain and its membranes, but, by the ſtrong impreſſion they make on the *ſenſorium commune*, may tend to diſlodge or remove the cauſe of the diſeaſe.

SECT. III.

Of the motion of the fluids in thoſe veſſels of animals commonly called abſorbent.

Besides the ſmall veins, which are continued veſſels with the arteries, and terminate at laſt in the two *venæ cavæ*, there are others which take their riſe from the internal ſurfaces of the ſeveral cavities in the body and from the ſkin: and, as the fluids which theſe veſſels convey, cannot be impelled into them by the force of the heart or arteries, they have been thought to receive them by ſuction, and therefore have got the name of abſorbent or imbibing veins. In the guts we find two kinds of them, *viz.* the lacteal veins, and thoſe commonly called abſorbent; which laſt are alſo to be found upon the ſurface of the ſkin, *peritonæum*, *pericardium*, *pleura*, veſicles of the lungs, *dura et pia mater*, and, in ſhort, of every membrane which lines any cavity of the body. In accounting for the motion of the fluids in theſe veſſels, we ſhall begin with the lacteals; in order to which it may be neceſſary to premiſe,

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[*] Hiſtoire de l'acad. des ſciences, 1708 & 1717.

[†] Van Swieten comment. in Boerhaave, aph. § 11.

1. THAT the lacteal veins have their origin in the villous coat of the guts, where their orifices are ſo ſmall

as to eſcape the eyes of anatomiſts: leaving the poſterior ſurface of the villous, they paſs through the nervous and muſcular coats, and, uniting into larger canals, are diſtributed in the form of a net-work in the external cellular membrane of the guts: after this, they enter the meſentery, and get valves, which hinder the return of any thing to the inteſtines.

2. As often as the muſcular coat of the guts is contracted, the lacteal veins, which paſs between the interſtices of its fibres, and are diſtributed in the nervous and external cellular membranes, muſt neceſſarily be compreſſed; but are relaxed and freed from this preſſure, when this coat ceaſes to contract.

3. MANY and repeated experiments have ſhewn, that ſmall glaſs tubes are endowed with a power, by which they attract fluids, ſo as to raiſe them conſiderably above the liquors in which they are immerſed—That this

power increaſes exactly in the inverſe *ratio* of their diameters—That theſe tubes, whether ſtraight or crooked, in a perpendicular or oblique poſition, in *vacuo* or the open air, attract fluids to the ſame height, provided their diameters be equal—That, when a capillary glaſs tube ends in a larger canal, the fluid is elevated ſo as to fill the capillary, but does not aſcend any further—That, if the diameter of a glaſs tube exceeds $\frac{1}{10}$ of an inch, its power of attraction is ſcarcely perceivable: and laſtly, that the ſame glaſs tubes attract different fluids to different heights, and this neither in proportion to their tenacity nor gravity. From all which it is natural to conclude, that the lacteal veins, which, in their beginning at leaſt, are ſmaller than any glaſs tubes made by human art, muſt be endowed with a remarkable power of attracting the chyle, when applied to their orifices.

How far the attractive power in ſuch canals, as the lacteals and other abſorbent veins, is, *cæteris paribus*, greater or leſs than in glaſs tubes, we have no experiments to determine: but, as the urine, an animal liquor, is more ſtrongly attracted by glaſs capillaries, than water or any other fluid*; it is not unreaſonable to ſuppoſe that animal capillaries may be endowed with a ſtill ſtronger power of attracting it. And, as the ſame fluid is differently attracted by capillary glaſs tubes of different natures, tho' of the ſame diameter†; is it not probable, that the ſeveral abſorbent veins in animals may be peculiarly fitted to attract their proper liquors moſt ſtrongly?

FURTHER, the remarkable attractive power with which the ſmall veſſels of vegetables are endowed, and by means of which they draw out of the ſame moiſt earth very different juices, is a ſtrong argument for allowing a ſimilar

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* Muſchenbroeck de tub. capill. vitr. cap. 3.

† Muſchenbroeck. Element. philoſ. natural. cap. xviii. § 531.

milar attraction in the veſſels of animals. It is by this power that the ſap continues to riſe in the veſſels of trees, even in the cold ſeaſon of winter, tho' ſlowly and in ſmall quantity: nor can it be pretended, that the ſun's heat promotes the aſcent of the ſap here, as it does in ſummer; ſince trees in cold cloudy weather, provided it be dry, and in places which the winter ſunbeams cannot reach, take up continually, by their roots, as much moiſture as is neceſſary to ſupply the waſte by perſpiration in their trunks and branches. But further, Dr. *Hales* has obſerved, that cut branches will imbibe from the ſmall end immerſed in water to the great end, as well as from the great end immerſed in water to the ſmall end [*]: whence it clearly follows, that the aſcent of the ſap in the veſſels of plants, is not owing to any peculiar ſtructure in them, but ſolely to capillary attraction.

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[*] Statical Eſſays, vol. I.

'Tis true indeed that capillary attraction, tho' it muſt make the ſap riſe in plants, will not, without the aſſiſtance of ſome other cauſe, make a continued derivation of it from their roots to their branches and leaves; becauſe as ſoon as capillary tubes are filled, or have raiſed fluids to a certain height, all motion from attraction ceaſes: but as the action of the air and ſun-beams upon the trunks, branches and leaves of trees, occaſions a ſtrong perſpiration of the ſap by their pores; a proportional quantity will be attracted from the earth by their roots, to ſupply this waſte and keep the capillary veſſels always full. However, as often as the abſence of the ſun and the cool moiſt ſtate of the air put a ſtop to the perſpiration of vegetables, the ſap ceaſes to aſcend; nay, if the earth be warm and dry, it gets a retrograde motion: and hence it is that, in a cool ſummer's evening when the dew begins to fall, vegetables attract the watery particles in the air by the pores of their leaves and branches, in like manner as they had done the moiſture of the earth by their roots, in the day time*.

THESE things being premiſed, it will be eaſy to account for the imbibition of the chyle by the lacteal veins.

WHEN any portion of the inteſtines is relaxed, the lacteal veſſels, whoſe open mouths are every where to be found on the ſurface of the villous coat, take in the chyle by their attractive power, ſo as to fill their branches which are diſperſed in the nervous and external cellular membranes of the gut. The chyle being thus received into the capillary lacteals, is by the ſucceeding contraction of the muſcular coat of the inteſtine, which compreſſes them, puſhed on towards the meſentery. As ſoon as this contraction ceaſes, the emptied lacteals, being free from compreſſion, fill themſelves with chyle as before, which the ſucceeding conſtriction of the gut preſſes forward to the larger lacteals in the meſentery. And thus we ſee the chyle is by turns attracted and propelled by the capillary power of the lacteals and periſtaltic motion of the inteſtines.

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* Vid. *Hales's* Statical Eſſays vol. 1.

Further, it is probable that the lacteal veins are, like the other ſmall veſſels of animals, agitated with a vibratory motion, excited in them by the gentle irritation of the chyle, which muſt aſſiſt the alternate contractions of the inteſtines in the propulſion of this fluid. Without allowing ſuch a vibratory motion in the umbilical veins of the chick, it will be no eaſy matter to account for its growth during the time of incubation. 'Tis true, the umbilical arteries and veins run cloſe together in oviparous as well as viviparous animals, ſo that the alternate pulſations of the former muſt contribute to the propulſion of the fluids in the latter towards the heart. But, as there is no pulſation to be obſerved in the heart or umbilical arteries of the chick, till towards the end of the ſecond day *; and as, at any rate, this does not extend beyond the red capillaries; the fluids in the extreme branches of the umbilical vein, muſt owe their motion to ſome other cauſe. And is it not reaſonable to think that the colliquated white is conveyed thro' theſe veſſels by their attractive power, as capillary tubes, aſſiſted by the ſmall alternate contractions excited in them by the gentle *ſtimulus* of this warm fluid? And in this opinion we are confirmed by the analogy of plants; in whoſe veſſels the circulation of the ſap is greatly aſſiſted by a vibratory motion, which ſeems to be excited in them chiefly by the ſun's heat. And is not the remarkable force of the ſap in the bleeding vine, owing to its veſſels being ſuſceptible of much ſtronger vibrations than thoſe of moſt other plants? †.

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* Malpigh. de ovo incubato.

† Dr. *Hales* has obſerved that, in a ſtem of a vine

The chyle in the larger lacteal veins which run along the meſentery and are provided with valves, is puſhed on to *Pecquet's* receptacle by the force of the new chyle continually tranſmitted to them from the guts, by the pulſation of the ſanguiferous arteries which run contiguous with them, and by the alternate motion of the diaphragm and abdominal muſcles in reſpiration.

If the chyle is received into the naſcent lacteal veins of the guts by their attraction as capillary tubes, it will be eaſy to ſee why quick-ſilver, which is repelled by ſuch tubes, ſhould, when ſwallowed by itſelf, generally paſs thro' the inteſtines without, almoſt, any of it getting into the blood.. On the other hand, if the propulſion of the chyle is owing to the alternate contractions of the guts, it may eaſily appear, why it

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vine $\frac{7}{8}$ of an inch diameter, the force of the ſap in the bleeding ſeaſon was five times greater than the force of the blood in the crural artery of a horſe. Statical Eſſays vol. 1. exp. 36.

it ceaſes to be tranſmitted thro' them ſoon after death; and why, in a well-fed animal newly killed, the lacteals in the meſentery, after being emptied, may be filled again, by gently preſſing the inteſtines and imitating their periſtaltic motion.

WITH reſpect to the abſorbent veins of the guts; the finer parts of the digeſted aliment received into them by their attraction, are propelled towards the larger meſeraic veins and *vena portarum*, by the alternate contractions of the muſcular coat of the inteſtines and preſſure of the abdominal muſcles and diaphragm in reſpiration. But, as theſe abſorbents are not provided with valves, like the lacteals, it may be aſked, Why the laſt-mentioned power does not preſs the abſorbed fluids equally backward to the guts, as forward to the *vena portarum*. This we imagine is prevented

1ſt, BY the gentle alternate contractions of the abſorbent veins, which, as they are owing to the *ſtimulus* of the imbibed liquor, muſt begin at their orifices and proceed towards their larger trunks. Such a motion as this, tho' gentle, will determine the courſe of the fluids on to the larger veins, but oppoſe their return to the guts. And we find in fact, that, by means of a ſimilar motion in the inteſtines, the uſeleſs part of the aliment is conveyed to the great guts, even in a horrizontal poſition of the body, where the alternate preſſure of the diaphragm and abdominal muſcles ought to puſh the contents of the guts as much backwards to the ſtomach as forward to the *colon*. But,

2. WHEN any portion of the inteſtines is contracted, the naſcent abſorbent veins, which riſe from the villous coat and paſs thro' between the other membranes of this part, muſt have their ſides preſſed together, ſo as to allow nothing to paſs thro' them; wherefore the preſſing force of the muſcles of reſpiration muſt, if acting

at this time upon the larger trunks of the abſorbent veins, propell their fluids towards the *vena portarum.* When this portion of the inteſtine is relaxed, the emptied abſorbents will, by their attraction, greedily fill themſelves with new fluids from its cavity: ſo that, whether the guts are contracted or relaxed, there will be always ſome obſtacle to the retrograde motion of a fluid in the abſorbent veins.

WHEN the liquors taken up by the capillary abſorbents are conveyed into the larger meſeraic veins, they will be carried along with *their* blood to the *vena portarum.*

As there are, upon the internal ſurfaces of all the cavities of the body, exhaling arteries which perpetually throw out a fine fluid to moiſten and lubricate the parts; ſo there are bibulous veins which take it up: whoſe exiſtence is proved, not only by no liquors being, in health, collected in theſe cavities, but alſo by anatomical injections*.

These abſorbent veins, which, like thoſe of the guts, have no valves, take up, by their attraction as capillary tubes, the rorid vapour of the arteries; after which, it is conveyed on to the ſanguiferous veins in which they terminate, by their vibrating motion, the pulſation of neighbouring arteries, and the compreſſion of muſcles. The abſorption in the cavities of the *abdomen* and *thorax* is greatly promoted by the alternate preſſure of the muſcles concerned in reſpiration; while the muſcles of voluntary motion employed in all kinds of exerciſe and labour, by accelerating the motion of the fluids in the abſorbent veſſels of the trunk and extremities of the body, enable them to imbibe more copiouſly. And hence we may ſee, why animals which move little, are generally oppreſſed with fat; while thoſe which are kept at hard

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* See Kauu perſpiratio Hippocrati dicta.

labour, are very lean. In the former, the abſorbent veins of the fatty cells imbibe the oily matter depoſited there very ſlowly, becauſe they want the alternate preſſure of the muſcles of voluntary motion to puſh their contain'd fluid forward to the larger veins: In the latter, the abſorption from thoſe cells is not only increaſed by the various and continually repeated preſſures of the acting muſcles, but, the body being, by much exerciſe, in ſome meaſure exhauſted of fluids, the veins imbibe more greedily, while the ſecerning arteries pour forth their oily liquor more ſparingly.

If the exhalant veſſels of any cavity throw out too much, or if the abſorbent power of the veins be weakened, or if both theſe happen together, a watery fluid will be collected in it; and in this way, are produced an *aſcites*, *hydrocele*, *hydrops pectoris*, *&c.*

When the blood is thin and watery and the veſſels weak, anaſarcous, œdematous and other dropſical ſwellings are common: for, as the bibulous veins can, by their attraction, only take up fluids in proportion to the depletion they ſuffer by means of their own vibratory contractions, and the alternate compreſſion of neighbouring arteries and muſcles; their abſorbing power muſt neceſſarily be leſſened in a lax ſtate of the fibres, where thoſe cauſes are much weakened.

FURTHER, while the redundance of a watery fluid in the blood increaſes the exhalation by the ſmall arteries, it leſſens the imbibition by the veins, for the ſame reaſon that aſhes, ſugar, or ſalts, when moiſtened, attract the watery particles of the air leſs ſtrongly than when they are dry.

AGAIN, altho' there be little or no fault in the blood itſelf, yet, if its return from any part to the heart be much retarded, a dropſy of that part will ſoon follow; becauſe the fluids taken up by the abſorbents, will be ſlowly and not without difficulty received into the larger ſanguiferous veins: and, as we have juſt now obſerved, their abſorption muſt be in proportion to their depletion. Hence we ſee, why ſchirrous tumors, ligatures, and whatever compreſſes the veins, ſoon bring on dropſical ſwellings.

It alſo appears from what has been ſaid, in what manner diuretics and purgatives carry off the ſtagnating waters in an *aſcites* and other dropſies: ſince, as, by the diſcharges they make by the kidneys and inteſtines, they not only leſſen the quantity of watery fluid in the blood, but alſo, by their *ſtimulus*, increaſe the force of the circulation; the exhalation by the arteries muſt be leſſened, at the ſame time that the imbibition by the veins is increaſed.

The ſurface of the ſkin and veſicles of the lungs are, like the other ſurfaces in the body, endowed with exhaling arteries and abſorbent veins:

by the former, there is perpetually diſcharged from the blood a fine lymphatic fluid; and, by the latter, the watery particles floating in the air are conſtantly conveyed into it.

WHEN the air is moiſt and the body has been exhauſted by fatigue, the imbibition by thoſe veins often exceeds the exhalation by the arteries; as Drs. *Keill* and *Linning* have obſerved*: but,

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* Medicin. Stat. Britain. tab. iv. & obſervat. & Philoſoph. tranſact. No. 470.

The remarkable imbibition by the ſkin obſerved by Dr. *Linning*, *July* 3. 1740, betwixt $2\frac{3}{4}$ and $5\frac{1}{8}$ afternoon, happened, 'tis true, without any preceeding fatigue; but is eaſily accounted for, from his having, in that time, diſcharged $28\frac{6}{8}$ ounces of urine: ſince ſo great a waſte of the thinner parts of the blood muſt not only have diminiſhed the exhalation by the cutaneous perſpiring arteries, but alſo have increaſed the abſorbent power of the imbibing veins every where thro' the body: and hence it is, that in a *diabetes* the urine often not only exceeds the quantity of liquors drunk, but taking the whole year round, the perſpiration made by the ſkin and lungs exceeds their imbibition by about forty ounces a-day in *Great Britain*, and fifty four ounces in *South Carolina*; which, tho' it has been commonly reckoned the total of the perſpiration, is really no more than its exceſs above the quantity of fluid taken in by the abſorbent veins of the ſkin, *fauces*, and lungs.

ALTHO' in vegetables, the veſſels, which perſpire in the heat of the day, frequently aſſume a contrary office in the night-ſeaſon, and imbibe the dew and watery particles then floating in the air; yet it does not ſeem probable, that the exhaling or perſpiring veſſels of animals can thus become imbibing ones, or that the moiſture of the

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theſe are taken up ſo greedily by the abſorbent veſſels of the ſtomach and guts, as to be diſcharged by the kidneys, before one would have thought they had got into the blood.

the air can be, by them, conveyed into the blood: ſince any motion in theſe veſſels, from their extremities to their larger trunks, muſt be in oppoſition to the courſe of the arterial fluids.

THE imbibition by the veſſels of the ſkin is performed in the ſame manner as in the other abſorbents; only it is probable, that the perpetually varying oſcillations of the external air may concur in promoting it.

ALTHO' the exhalations from animal, vegetable and mineral bodies, may be tranſmitted, along with the watery particles in the air, into the blood, by the abſorbent veins of the ſkin and lungs, and thus account for peſtilential and epidemical diſeaſes raging at particular ſeaſons; yet it is by no means probable, that *elaſtic* air can be imbibed by theſe veſſels, and thus conveyed into the blood: for it has been obſerved, that this fluid moves with great difficulty thro' capillary glaſs tubes, tho' ſome hundred times larger than the pores of the ſkin [*]: and it is well known, that water and other fluids can penetrate many ſubſtances, thro' which air cannot paſs.

THIS obſervation of the difficulty with which air moves thro' capillary tubes, may ſerve to determine a controverſy which has long ſubſiſted among phyſiologiſts: *viz.* Whether or not any *elaſtic* air enters into the blood by the lungs. For, ſince a few drops of water, with ſmall portions of air between them, in a capillary tube, require a greater force to make them aſcend, than that with which the tube attracts the particles of that fluid [†]; it muſt follow, that, if any *elaſtic* air were admitted into the abſorbent veins of the lungs, it would not only not move thro' them itſelf, but hinder their taking up, by their attraction, any other fluids.

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[*] Aërem vero non niſi tardè et cum quadam tenacitate per hos tubos moveri, ſemper docuit experientia; aëri enim ineſt ſpecies quaedam tenacitatis aut immobilitatis. Muſchenbroeck De tub. capill. vitr. cap. 1. exp. xi.

[†] Muſchenbroeck loc. citat.

THE prodigious ſwelling of animals in an exhauſted receiver, further ſhews, that air cannot readily paſs thro' the ſmall pores of the ſkin and lungs. Nor is it any objection to this doctrine, that air has been found in the cavities of the heart; ſince, in a morbid ſtate, this might ariſe from the blood, of which air is a conſtituent part, as well as of other fluids *.

IT is very obſervable, that air injected into the veins of an animal, produces obſtructions, concretions and ſudden death; which effects, however, may be eaſily accounted for, from the power which air has of coagulating blood, and from the ſurpriſing influence it has in ſtopping the motion of water, even in large pipes, eſpecially when lodging in their flexures †.

BUT,

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* *Hales*'s Statical Eſſays, vol. 1. chap. vi.

† Philoſoph. tranſact. No. 393.

But, to return; as the effluvia of different ſubſtances floating in the air, are, by means of the cutaneous abſorbents, conveyed into the blood, ſo likewiſe are the finer parts of plaiſters, cataplaſms, fomentations, and all other external applications: which ought therefore to be conſidered, not only as having a topical influence, but alſo as acting upon the whole body by their ſubtiler parts, which are mixed with the blood and other fluids.

It may be thought a difficulty, that quick-ſilver applied in the form of an ointment, ſhould be taken in ſo readily by the abſorbent veſſels of the ſkin; ſince, as has been obſerved above, it paſſes thro' the inteſtines without getting into the lacteals. But this happens from the particles of the mercury being extremely divided, and ſo united with thoſe of the greaſe as to enter the pores of the ſkin along with them: for, tho' quick-ſilver is repelled by capillary glaſs tubes, yet, if their internal ſurface ſace is run over with melted greaſe, it will be attracted by them *.

We are told, that, upon opening the bodies of ſuch as had taken mercury in large quantities, this fluid has been, ſometimes, found in the cellules of the bones and elſewhere †; the reaſon of which may be eaſily underſtood from what has been ſaid above: for, if the very ſubtile and greatly divided particles of mercury ſhould, after they are thrown out, by the exhaling arteries, into any cavity of the body along with the finer parts of the blood, unite by their ſtrong mutual attraction, ſo as to form globules whoſe diameters are larger than the diameters of the abſorbent veins, 'tis evident, they could never be taken up by theſe veſſels, but muſt remain for ever in ſuch cavity.

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* Memoires de l'academ. des ſciences an. 1724. and Muſchenbroeck de tub. capill. cap. iv. exp. 12. cor. 2. and cap. vii.

† Wepfer de apoplex. p. 277, and Mead on poiſons, edit. 3.

To conclude our obſervations on the abſorbent veſſels of animals; It may not be improper to take notice, that there are, upon the internal ſurfaces of the follicles and ſecretory and excretory ducts of the glands, bibulous veins, whoſe office is to carry off fluids which would be improper to enter into the ſeveral ſecretions. And, if we ſuppoſe theſe abſorbent veſſels, like other capillary tubes, to attract, according to their different natures, different fluids more or leſs ſtrongly, we ſhall ſee one great cauſe of the various ſecretions performed in the bodies of animals.

II.

OBSERVATIONS

ON THE

SENSIBILITY and IRRITABILITY

OF THE

Parts of MEN and other ANIMALS.

Occaſioned by Dr. *Haller*'s late Treatiſe on theſe Subjects.

Spiritus intus alit; totamque infuſa per artus
Mens agitat molem—— VIRG.

OBSERVATIONS

ON THE

SENSIBILITY AND IRRITABILITY

OF THE

Parts of MEN and other ANIMALS.

PART I.

Of Senſibility.

THE truly learned and juſtly eſteemed Dr. *Haller*, in his late treatiſe *De partibus corporis humani ſenſibilibus et irritabilibus* *, has favoured the world with an account of many new and curious experiments; from which he has frequently drawn ſuch concluſions, as, if juſt, muſt neceſſarily produce conſiderable changes both in the theory and practice of the medical art. Being ſenſible how contrary his doctrine is, in many things, to the received

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* Acta Gottingenſ. vol. 2. ad an. 1752, pag. 114, &c.

ceived opinion of almoſt every phyſician, antient as well as modern, he has been at uncommon pains in making many and repeated experiments; as much to overpower the incredulous by their number, as to ſecure himſelf from any chance of being deceived*.

Opinions, *even* purely theoretical, ſhould not be let paſs, if there is any fallacy in them: but, when propoſitions, founded on experiments, and ſupported by men of high character, are advanced, by which practitioners in medicine may be led into errors; it becomes the duty of every lover of the healing art, to prevent their being generally received as truths.

If the concluſions in the treatiſe above quoted, ſhall be thought juſt, phyſicians and ſurgeons will certainly treat their patients in a manner very different from what they have hitherto done; whereby, if there be a miſtake in the doctrine, many lives may be endangered or loſt. It ſeems to be of ſome conſequence, therefore, to conſider this matter with attention, and to examine particularly, How far Dr. *Haller's* ſyſtem of ſenſibility is, or is not, well-founded.

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* Acta Gottingenſ. p. 115.

SECT. I.

OUR author, in treating of the ſenſibility of the ſeveral parts of the human body, reckons, among the inſenſible parts, the tendons, *aponeuroſes*, ligaments, *capſulæ* of the articulations, *perioſteum*, bones, marrow, *dura* and *pia mater*, *pleura*, *peritonæum*, *pericardium*, *mediaſtinum*, and *cornea*.

1. HE tells us, that living animals, whoſe tendons were cut, burnt, pricked, or torn, ſhewed no ſigns of uneaſineſs; and, when a little part of the *tendo Achillis* was left intire, they walked without any ſeeming pain [*].

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[*] Act. Gotting. vol. 2. p. 120.

2. When the ligaments and *capſulæ* of the articulations were pricked with a needle, ſcraped with a knife, or had oil of vitriol or *butyrum antimonii* applied to them, the animals ſhewed no ſenſe of pain [*]. The wounds of theſe parts and of the tendons were followed with no bad ſymptoms, and were cured without any other remedy than the *ſaliva* of the animal, and ſometimes without this [†].

3. The *perioſteum*, when wounded, torn or burnt, cauſed no pain to the animals[‡].

4. He allows feeling to the teeth, but not to the other bones, becauſe they are not furniſhed with nerves, and becauſe he has ſeen the ſkull trepanned, without giving pain, in perſons who were poſſeſſed of all their ſenſes[‖].

5. He denies feeling to the marrow, not from any experiments of his own on living animals; but becauſe it is a fatty ſubſtance and deſtitute of nerves [§].

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[*] Act. Gotting. vol. 2. p. 122 et 123.

[†] Ibid. p. 121 et 223.

[‡] Ibid. p. 123.

[‖] Ibid. p. 124.

[§] Ibid. p. 125.

6. When the *dura mater* was cut or lacerated, or burnt with oil of vitriol, ſpirit of nitre, and *butyrum antimonii*, the animal ſeemed to have no feeling of the injury *.

7. When the *pia mater* was burnt by touching it with *butyrum antimonii*, the animals neither cried, nor were they convulſed; but, as ſoon as the brain itſelf was wounded, the body of the animal was twiſted and diſtorted with violent convulſions †.

8. The *peritonæum*, *pleura*, and *pericardium*, when laid bare and cut, or otherwiſe irritated, produced no change in the animal ‡.

9. He denies feeling to the *mediaſtinum*, not upon the authority of any experiments, but becauſe, like the *pleura*, it is a membrane and deſtitute of nerves ‖.

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* Act. Gotting. vol. 2. p. 126.

† Ibid. p. 130. ‡ Ibid. p. 130.

‖ Ibid. p. 131.

10. He reckons the *cornea* inſenſible, becauſe its nerves cannot be demonſtrated, and it is often pierced with a needle without giving pain *.

Besides the inſenſible parts above mentioned, there are others which, according to Doctor *Haller*, have either no ſenſe of feeling, or a very obſcure one; and theſe are the arteries, veins, glands, and *viſcera*, *viz.* the lungs, liver, ſpleen and kidneys, which, when pricked, cut, or otherwiſe irritated, ſhewed nothing like feeling †.

The concluſions which our author draws from the above experiments, may be reduced to the three following.

1*ſt*, That the tendons, ligaments, *capſulae* of the joints, *dura mater*, *pleura* and other membranes, are quite inſenſible.

2*dly*, From the inſenſibility of theſe parts, and the difficulty of tracing, by diſſection, any nerves to them, he concludes that they have none, and that this is the reaſon why they are deſtitute of feeling.

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* Act. Gotting. vol. 2. p. 133.

† Ibid. p. 131 and 132.

3*dly*, HE thinks it follows, that thoſe parts which, from his experiments, he concludes to be inſenſible, have been unjuſtly accuſed by phyſicians, as the ſeat of many painful diſeaſes. Particularly, that the pain, ſwelling and inflammation which have often followed venæſection in the flexure of the arm, have not been owing to the tendons or *aponeuroſes*, in that part, being pricked by the lancet, but to the median nerve or ſome branch of the muſculo-cutaneous nerves being wounded *.—That we need be no way afraid of wounds of the tendons, whether they be cut, pricked, burnt, or otherwiſe hurt.——That the *cephalæa* and *phrenitis* have not their ſeat in the *dura mater*. †.—That the ſkin or ſubcutaneous nerves are the ſeat of the violent pain with which arthritic patients are affected, and not the ligaments or *capſulæ* of the joints *. And that the pain of the pleuriſy has been without reaſon ſuppoſed to be owing to an inflammation of the *pleura*, which is void of feeling †.

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* Act. Gotting. vol. 2. p. 121.

† Ibid. p. 126.

In the few obſervations which I propoſe to make on this doctrine, I ſhall, *Firſt*, Conſider the parts, reckoned inſenſible by Dr. *Haller*, in a ſound natural ſtate, ſuch as they were in his experiments; and 2*dly*, When they are affected with diſeaſes, whether in conſequence of ſuch experiments, or from other cauſes.

SECT. II.

1. In making or relating experiments, with a view to diſcover the ſenſibility or inſenſibility of the ſeveral parts of animals, particular regard ſhould be had to an obſervation made by *Hippocrates*,

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* Act. Gotting. vol. 2. p. 122. and 123.

† Ibid. p. 130.

pocrates, above two thouſand years ago, *viz.* That a greater pain deſtroys, in a conſiderable degree, the feeling of a leſſer one *; an obſervation, the truth of which is confirmed by the daily experience of every phyſician. Thus, pricking any part of the body ſo as to give conſiderable pain, will ſo obliterate the irritation in the left orifice of the ſtomach, which is the cauſe of the hiccup, as inſtantly to put a ſtop to this convulſive motion. If a lighted candle be brought near a perſon whoſe eyes are a little inflamed, it will give him a good deal of uneaſineſs; but, if he be placed firſt in the ſunſhine, the candle will not add ſenſibly to his pain.

WHEN a frog's hinder-feet are pricked or otherwiſe wounded, immediately after cutting off its head, it makes ſcarce any motions at all with its legs, and ſhews almoſt no ſigns of feeling; but, if the toes are pricked or cut, ten or fifteen minutes after decollation, the legs and thighs are not only violently moved, but ſometimes alſo the trunk of the body. Now, if in this caſe, as we ſee, the great pain occaſioned by cutting off the head rendered the animal for ſome time inſenſible, when its toes were wounded; is it to be wondered at, that, after the more ſenſible parts were cut, thoſe animals, which Dr. *Haller* opened, ſhewed no ſigns of pain, when the leſs ſenſible parts were wounded?

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* Aphor. Lib. 2. No. 46.

When the *thorax* of a living animal is laid open, it does not ſeem to receive any additional pain by pricking or cutting its heart; no new convulſions are produced, nor any change in the body, except perhaps a quicker repetition of the heart's motions: does it follow from this, that the heart is deſtitute of feeling? No, ſurely; but only that, after the great tortures ſuffered by laying open the *thorax*, the new pain produced by wounding the heart is too ſmall to make any remarkable impreſſion upon a dying and half-inſenſible animal.

DOES it not appear, from what has been ſaid, that a want of due attention to the above-mentioned maxim of *Hippocrates*, which is ſupported by the ſtrongeſt experiments and obſervations, has given occaſion to Dr. *Haller's* miſtakes with regard to the ſenſibility of many of the parts of animals? Thus, it will not follow, that the tendons, ligaments, *capſulæ* of the joints, *perioſteum* and *dura mater*, had no feeling at all when they were cut, torn or pricked, becauſe no convulſive motions or other ſigns of uneaſineſs appeared in the animals at that time; for this might be owing to the greater pain occaſioned by cutting the ſkin, ſubcutaneous nerves, &c. in order to get at thoſe parts, the ſenſibility of which our learned author propoſed to try. The concluſion therefore which ſhould be made from his experiments, is, not that the parts above-mentioned are wholly deſtitute of feeling, but that they are much leſs ſenſible than ſome others, or than has been commonly believed by phyſicians.

WITH regard to the marrow which Dr. *Haller* reckons inſenſible; *Duverney*'s experiments made on men *, (which have alſo ſucceeded with my ingenious friend and collegue Mr. *Monro*) and particularly his experiment made on a living animal before the Royal Academy of Sciences at *Paris* †, are ſufficient proof that this part is far from being deſtitute of feeling: and the reaſons given by Dr. *Haller* for his placing it among the inſenſible parts, are not of any weight, when compared with thoſe experiments; for the feeling of the marrow is not owing to its oil, but to the membranes containing this oil: and the experiments which demonſtrate its ſenſibility, prove that theſe membranes are furniſhed with nervous filaments, altho' they may be too ſubtile to be traced by the knife of the moſt accurate anatomiſt.

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* Dans les hôpitaux, où voyant panſer ceux qui avoient eu un bras ou une jambe coupée, je pouvois voir la moëlle à decouvert,—toutes les fois que je la faiſois toucher un peu rudement, le malade donnoit auſſi-tot des marques d'une nouvelle douleur.

Memoires de l'Acad. des Sciences 1700. edit 8vo, p. 255.

† "Vous vous ſouviendrez, Meſſieurs, que "je fis ſcier devant vous, par le milieu, l'os de "la cuiſſe d'un animal vivant; et, ayant fait "ôter les chairs et les membranes pour laiſſer "le

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" le bout de l'os entierement à nud, comme " tous ces ebranlemens et ces diviſions cau- " ſoient de douleur tres cruelle a l'animal, " j'eus la precaution d'attendre que cette " douleur fût paſſée, et, quelque tems après, " plongeant un ſtilet dans la moëlle, vous vites " que l'animal donna auſſi-tôt des marques " d'une tres vive douleur, ce qui fut reiteré " pluſieurs fois avec la meme precaution, et " avec le meme ſuccès."

Memoires de l'Academie Royale des Sciences 1700, edit. 8vo, p. 256.

3. THE *tunica cornea* is ſo far from being inſenſible, as Dr. *Haller* would perſuade us, that any one may be ſoon convinced of the contrary by an eaſy experiment upon his own eye; for, when the *cornea* is touched with the point of one's finger, a very ſenſible pain is felt: and it is well-known, that powder of tobacco, or any acrid liquor applied to the *cornea*, excites a very painful ſenſation. Tho' the ſclerotic coat of the eye is far from being deſtitute of feeling, yet I have found it to be leſs ſenſible than the *cornea*; by touching both, not only with the point of my finger, but alſo with a bit of ſoft ſilk or linnen.

HAVING had lately occaſion to be preſent at the extraction of the cryſtalline *lens* in Mr. *Sharp*'s way *, I enquired particularly at the patient, Whether he felt any pain when the *cornea* was firſt pierced with the knife employed in that operation: he told me, He thought the pain was much the ſame with what he uſed to feel when the ſkin of his arm was cut in blood-letting. It deſerves however to be remarked, that, tho' the ſkin and *cornea* are both endowed with a very conſiderable degree of ſenſibility; yet, when they are cut quickly with a very ſharp inſtrument, there is much leſs pain felt than one would imagine. Thus, when the ſkin is ſlightly wounded in ſhaving one's beard with a razor, the blood that follows is often the firſt thing that lets one know of any ſuch thing having happened: and this, together with the pain occaſioned by holding the eye firm in its orbit, and the concern the patients are generally in, may very well account for their ſcarce perceiving any pain when the *cornea* is pierced with a ſharp needle. So that, upon the whole, it appears, that the *cornea* is poſſeſſed of a remarkable degree of ſenſibility; and conſequently, that Dr. *Haller*'s poſition, That all membranes are deſtitute of feeling*, muſt admit, at *leaſt*, of one exception.

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* Philoſoph. tranſact. vol. xlviii. p. 1. p. 322.

4. Our author allows the kidneys either no feeling, or a very obſcure degree of it; becauſe he could obſerve no ſigns of pain in the animals, whoſe kidneys he cut or pricked with a knife: but, after cutting the ſkin, abdominal muſcles, &c. and diſplacing the inteſtines in order to get at the kidneys, it was ſcarcely to be expected, that the animals would ſhew any tokens of additional pain when theſe organs were wounded, unleſs they had been equally, or more, ſenſible, than the parts before diſſected.

A phyſician of my acquaintance, who had occaſion to ſee the operation of nephrotomy performed a few years ſince, was told by the patient that, when the kidney was opened, he felt pain, tho' duller and leſs acute than when the ſkin was cut.

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* Act. Gotting. vol. 2. p. 130.

It is very obſervable that, while Dr. *Haller* denies feeling to the kidneys, he allows it to the ureters: not becauſe animals, when theſe are cut or wounded, ſhew ſigns of greater pain, than when the kidneys are treated in the ſame manner; but becauſe he ſuppoſes the ureters to be of the nature of the ſkin, and propagated from it *. And indeed, even, the ſtrongeſt experiments upon brute animals would not have been ſufficient to have proved the ureters inſenſible in men; when ſtones paſſing from the kidneys to the bladder, generally occaſion ſuch exquiſite pain. But, does not the acute pain always attending a *nephritis*, and ſometimes occaſioned by a ſtone lodged in the kidneys, ſhew beyond doubt, that they are endowed with feeling, as well as the ureters? while nothing can be concluded from *calculi* lying long in the kidneys without giving pain †, except that they were ſo ſituated as not to hurt them.

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* Act. Gotting. vol. 2. p. 131.

† Act. Gotting. vol. 2. p. 132.

5. ALTHO' brute animals ſhew ſmall ſigns of ſenſibility, when glands are pricked, or have acrid things applied to them, immediately after the very ſenſible ſkin has been cut; yet, we know that a bruiſe on the teſticle often cauſes, inſtantly, ſuch exquiſite torture, as to make men faint; and a blow on a woman's breaſts often excites, immediately, ſhooting pains in the gland there, tho' no mark of the bruiſe appears in the ſkin. Theſe are ſuch undoubted proofs of the ſenſibility of the glands, as no experiments made on brute animals will ever be able to overthrow.

6. DR. *Haller* allows the membranes of the *aorta* near the heart, and of the temporal, lingual, labial, thyroid and pharyngean arteries, to be ſenſible; but thinks the coats of the arteries in other parts of the body have either no feeling or a very obſcure degree of it: tho' it does not appear from his experiments,

that animals complained more when the former, than when the latter, were irritated. In this caſe, he relinquiſhes the appeal to experiment, and founds his opinion on his tracing nerves to the former, which he could not do to the latter: an argument he makes uſe of upon ſeveral other occaſions, and which is next to be examined.

7. As our author not only founds his opinion of the inſenſibility of many parts of the body upon experiments made on living animals, but, alſo, on their being deſtitute of nerves; we ſhall briefly conſider, whether, from the real or ſeeming inſenſibility of any part, or from anatomiſts being unable to demonſtrate its nerves, we are intituled to conclude that it has none.

ALTHO' the tendons are quite inſenſible according to Dr. *Haller*, and their nerves can ſcarcely be demonſtrated by anatomiſts; yet, we are convinced, that the tendons are not deſtitute of nerves, from the following obvious obſervation. In fœtuſes and new-born children, the parts which, afterwards, in an adult ſtate, become tendinous, are muſcular or partly ſo; and, as animals advance in age, the proportion of the tendinous, to the muſcular part, gradually increaſes: we muſt either, therefore, deny nerves to the muſcles, or allow them to the tendons alſo.

Altho' we cannot trace nervous filaments to the ſmall arteries, we have reaſon to believe they are furniſhed with them; elſe, how could the diſtraction of their coats in inflammations, occaſion ſuch acute pain? I think we may conclude every part that is liable to be inflamed by irritation, to be, in ſome degree, ſenſible and endowed with nerves; for, ſince the inflammation cannot in this caſe be owing to any increaſed force of the heart, the diſtenſion of the ſmall arteries, and the greater *impetus* of the blood in them, muſt be owing to an increaſed oſcillatory motion in the veſſels themſelves, excited by the unuſual irritation: but, theſe motions of the ſmall veſſels being of a like kind with thoſe alternate contractions which are obſerved in muſcles whoſe fibres have been irritated, it will follow that thoſe veſſels partake of a muſcular nature, and conſequently have nerves like the other muſcles.

WITH regard to the membranes; ſince the *dura mater* and *pleura* are furniſhed with nervous filaments, which anatomiſts have been able to demonſtrate *, we may reaſonably conclude that the other membranes are not deſtitute of them; altho' they may be too ſmall to come under the eye of the beſt diſſector: this is certainly true of the *cornea* and membranes containing the marrow, which we have ſhewn, from undoubted experiments, to be ſenſible, and conſequently not without nerves. It appears therefore, that we can by no means conclude any part to be inſenſible, *merely* becauſe its nerves cannot be demonſtrated.

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* Winſlow expoſition. anatom. ſect. ix. No. 35. and ſect. x. No. 47.

On the other hand, it is allowed that we cannot certainly conclude, from a part's being furniſhed with nerves, that it is ſenſible at all, or in what degree: for, the nerves muſt be in a certain degree of flexibility and tenſion, to perform their offices rightly; and, in proportion as they recede from this, their ſenſibility will be more or leſs blunted. Examples will illuſtrate this.

The bones, which in a natural ſound ſtate are inſenſible, are nevertheleſs moſt certainly furniſhed with nerves; as appears from the remarkable ſenſibility of the granulated ſubſtance which riſes from them after fractures, or their being chizelled, or when they expoliate: this ſoft fleſh, however, gradually loſes its feeling as it grows harder, till being, at laſt, turned into a callous or bonny ſubſtance, it becomes wholly inſenſible.

The membranes of the *tela cellularis* are, in a natural ſtate, ſoft, flexible and diſtenſile, and have little feeling; but, in every wound or ulcer, when they acquire ſome more firmneſs, they are ſenſible of every touch and every acrid application, as ſurgeons ſee daily. After a cicatrice has, ſometime, covered the parts where the ſore was, and they have returned to their natural ſoftneſs, theſe cellular membranes loſe again their ſenſibility, as appears on making a new wound thro' the cicatrice; and recover it again, whenever they become firm and tenſe, by the new inflammation and ſuppuration.

The *dura mater*, which, in a ſound ſtate, has but little feeling, granulates after the trepan, and feels every irritating ſubſtance applied to it; and the ſame thing happens to cartilages, ligaments, tendons, membranes, *&c.*

Without attention to this change in the firmneſs of parts, and its effect upon their nerves, we could never account for what has been obſerved above, *viz.* that the parts of muſcles, which in fœtuſes and children are lax contracting fibres and very ſenſible, become, in a great meaſure, inſenſible, in a ſound ſtate, when, by the creature's advancing in age, they are compacted into tendons, as happens to many of them.

If ſenſibility, then, be a ſure mark of the exiſtence of nerves in any part of the body, there is not one that is deſtitute of them, altho' anatomiſts will never be able to demonſtrate them in every part.

From what has been ſaid, it may appear, that Dr. *Haller's* experiments on living animals do not ſufficiently prove the doctrine he would deduce from them, and that his argument for the inſenſibility of parts, taken from their nerves not being demonſtrable, is altogether inconcluſive. Let us next try what further light diſeaſes will throw upon this ſubject.

SECT. III.

If the parts reckoned inſenſible by Dr. *Haller* were really deſtitute of nerves, it would follow that they could in no caſe become the ſeat of painful ſenſation; and even ſuppoſing them furniſhed with nerves, but poſſeſſed only of an obſcure degree of feeling, it may be thought, at leaſt, not probable that they can be the ſeat of thoſe painful diſeaſes commonly aſcribed to them. In order to ſet this matter in a proper light, it will be ſufficient to diſtinguiſh between parts in a ſound and in a diſeaſed ſtate. In a ſound ſtate, the feeling of many parts of the body is but very dull, which is altogether neceſſary to prevent the uneaſineſs we would otherways perpetually ſuffer, when our organs are ſtretched, preſſed upon, &c.

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in the common offices of life: ſuch parts, therefore, when cut or wounded, in a ſound ſtate, give little uneaſineſs; but, if afterwards an inflammation comes on them, they become extremely ſenſible, and their over-ſtretched veſſels and nervous filaments occaſion intenſe pain, by which we are excited to endeavour the cure of the diſeaſe.

It is certain, that the parts which are moſt ſenſible in a ſound ſtate, acquire a more acute feeling when inflamed. Thus the ſtomach, which, in health, can bear the touch of wine, brandy, and other pungent liquors, without being hurt, is, when inflamed, often brought into convulſions by the mildeſt drinks; and light, which gives no ſenſible pain to the eye in a ſound ſtate, becomes intolerable when this organ is inflamed. Nor can we doubt that the more inſenſible parts may acquire, when inflamed or otherways diſeaſed, a remarkable degree of ſenſibility.

lity. Examples above recited have ſhewn this to be true of the bones, *tela cellularis*, and *dura mater*; and the following facts will ſhew the ſame thing, in other parts, reckoned either wholly, or almoſt wholly inſenſible by our author.

As often as there is an inflammation, eſpecially when tending to ſuppuration, in any of the glands, as the parotids, tonſils, maxillaries, *mammæ*, *teſtes*, kidneys, *&c.* the patient is tortured with pain, often, before the teguments are affected or even conſiderably ſtretched. And is not this a much better proof of the ſenſibility of theſe parts, than *ſchirri* and other indolent ſwellings are of the contrary?

The fore-part of the eye, when inflamed, can bear the touch of no hard or acrid ſubſtance; and *fungi* riſing from it, give very ſharp pain, when fretted.

In the rheumatiſm, joints, where the ſkin is unſtretched and of the natural colour, and where no muſcular fibres are placed, are ſeverely pained on the leaſt motion, tho' done without the effort of the patient, which muſt therefore depend on the ſenſible ligaments and tendons; ſince large branches of nerves, thus affected, would produce convulſions of the muſcles they ſerve, which do not happen: beſides, in theſe caſes, the pain is not felt where the large nerves are.

A contuſion, by a fall on the great *trochanter* of the thigh, without cauſing *ecchymoſis*, or ſwelling of the teguments, often brings, in a little time, racking pain on all the outſide of the thigh, leg and foot; which continues obſtinately for months or years thro' the whole extent of the *faſcia lata*.

An inflammation of the *perioſteum*, as in the *panaris*, where the ſuppuration happens between this membrane and the bone, nay, even the repletion of the veſſels of an over-ſtretched *perioſteum*, as by heat or food in venereal nodes, gives very ſharp pain. And, in the *ſpina ventoſa* and other ſuppurations of the marrow, pain is felt before any ſigns of the diſeaſe appear externally.

THESE obſervations ſeem to demonſtrate, beyond doubt, that many of thoſe parts, which Dr. *Haller* would have us believe to be inſenſible, are often the ſeat of remarkable pain in the human body; and, I cannot help thinking, that, in other examples, where he endeavours to aſſign a different ſeat of the painful ſenſation, he is miſtaken, and is laying the foundation of dangerous practice. It will, therefore, be worth while to examine theſe caſes.

1. HE imagines that the pain, ſwelling, and inflammation of the arm, which have ſometimes followed the opening of the *median* vein, muſt have proceeded, not from a wound of the tendon of the *biceps* muſcle, but of the *median* or ſome other nerve. But, if

this were the caſe, why ſhould not ſimilar ſymptoms ſometimes follow blooding in the cephalic or jugular veins? In opening the jugular vein, ſome nervous filaments are frequently wounded, and often occaſion a ſharp pain, as if the point of the lancet had been left in the wound; this, however, goes off in a day or two, or ſooner, without leaving any bad conſequence. But the miſchiefs which have followed blooding in the *median* vein are of a different kind; tho' little or no pain is felt at firſt, yet afterwards, not only the whole arm is violently pained and ſwelled, but a particular hard ſwelling is often formed in the place where the wound was made, from which a thin lymph iſſues; and the patient does not recover the full uſe of his arm for ſeveral months; nay, ſometimes loſes the motion of the elbow-joint altogether. And, that a wound in the tendon is, at leaſt, ſometimes the cauſe of thoſe ſymptoms that follow blood-letting in the flexure of the arm, appeared evidently in a patient who died in this place, ſome years ago, of a fever occaſioned by the pain, ſwelling and inflammation, conſequent upon opening the median vein of the right arm, the *tendo bicipitis* of which was ſwelled to near ten times its natural bulk.

How very ſenſible tendons may become when inflamed, appears from various obſervations; particularly one mentioned by the learned Dr. *Van Swieten*, of a Nobleman, who was ſeized with moſt terrible convulſions over his whole body the moment his ſurgeon took hold of one of the tendons near his ancle, miſtaking it for a part of the fatty membrane*.

2. Our author aſcribes the pain of the gout to the ſkin or ſubcutaneous nerves, and not to the *capſulæ* or ligaments of the joints affected. But does not the rigidity of the joints, which

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* Comment. in Aphor. Boerhaave, vol. 2. p. 241.

the gout at laſt produces, ſhew, that its ſeat is deeper than the ſkin or nerves below it; and that the ligaments of the articulations, and tendons of the muſcles which ſerve for their motions, are affected?

When one ſprains his wriſt or ancle, there is often no great pain felt immediately; but ſoon after, when the overſtretched parts begin to ſwell and inflame, a conſiderable pain enſues, which is greatly increaſed if the joint be moved. Does not the pain in this caſe proceed chiefly from the overſtretched ligaments or tendons? It will be hard to perſuade phyſicians, that it is owing to any hurt received by the ſkin or ſubcutaneous nerves. And, if the ligaments or tendons may be affected with pain from being too much ſtretched, why may they not be the principal ſeat of that pain which affects the joints of gouty patients?

Chalk-stones in a joint frequently give ſharp pain before they pierce the capſular ligament, and before the ſkin is much ſtretched or red. Further, without allowing ſenſibility to the ligaments, let any one try to explain what my ingenious friend Mr. *Monro*, and, I dare ſay, many others, have oftener than once ſeen in practice. A pea-iſſue, for a dropſy of the knee, put in with a cauſtic or a knife, and dreſſed with the pea a conſiderable time, created little uneaſineſs to the patient; but, after a puncture of a lancet, made, very near to where the iſſue was, thro' the *capſula* of the joint to let out the water, moſt racking pain and inflammation enſued, which brought the patient to the brink of the grave.

3. Our author is of opinion, that the inſenſible *dura mater* cannot be the ſeat of a headach or *phrenitis*. But how little ſenſible ſoever this membrane may be in a natural ſtate, yet, if

it may be affected with pain, as often as it is inflamed or obſtructed, it may ſtill be, in many caſes, the ſeat of theſe diſeaſes. In patients who have died of a *phrenitis*, the *dura* and *pia mater*, as well as the cortical ſubſtance of the brain, have been found inflamed, ſuppurated, and mortified: and in thoſe who, after recovering once and again of a *phrenitis*, have died of other diſeaſes, the *dura* and *pia mater* have been found much thicker and harder than uſual. *

As the headach generally attending fevers often begins ſeveral days before any ſigns of a *delirium* appear, we cannot aſcribe it to an obſtruction in the cortical part of the brain, but in the *dura* or *pia mater*. Nor can this headach have its ſeat in the exterior teguments of the ſkull; otherways, the pain would be increaſed by preſſing the part chiefly affected, as often happens in thoſe periodical headachs which ſeem to have their ſeat in the ſubcutaneous nerves, or *pericranium*.

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* Van. Swieten comment. *vol. 2. p.* 604.

4. LASTLY, Dr. *Haller* thinks, that the intercoſtal muſcles, or large nerves running between the ribs, are the ſeat of the pain of the pleuriſy, and not the *pleura* itſelf, which is inſenſible. But, if this membrane, notwithſtanding its ſmall degree of ſenſibility in a ſound ſtate, may be affected with great pain, when inflamed; it will hardly be doubted that it is ſometimes the ſeat of the pleuriſy: ſince, in patients who have died of this diſeaſe, the *pleura* has been found inflamed and ſuppurated *.

BUT, beſides the inſenſibility of the *pleura*, Dr. *Haller* has brought another very plauſible argument to prove, that the pleuriſy can never have its ſeat in this membrane; *viz.* the patient's feeling the greateſt pain in inſpiration when the ribs are brought nearer each other, and conſequently when the *pleura* is leſs upon the ſtretch than it was in

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* Van Swieten comment. vol 3. p. 8.

in time of exſpiration. But the Doctor has long ago very juſtly obſerved, that ordinary and gentle inſpiration in men, is chiefly performed by the diaphragm, while the intercoſtal muſcles are ſcarce employed at all *: wherefore, in inſpiration, which pleuritic patients perform with great caution, the ribs may be ſuppoſed to alter their ſituation very little †; but, as the inferior part of the *pleura* muſt be ſomewhat ſtretched by the deſcent of the diaphragm in inſpiration, it is no wonder the pain ſhould be, then, moſt acute.

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* Praelect. in inſtitut. med. Boerhaav. vol. iv. No. 615. not. (*a*).

† It is ſomewhat ſurpriſing, that our learned author ſhould have mentioned the approach of the ribs in inſpiration, as an argument to prove, that the pleuriſy never has its ſeat in the *pleura*; after having formerly told us, that pleuritic patients don't uſe the intercoſtal muſcles at all, but breathe by means of the diaphragm alone. Praelect. in inſtitut. med. Boerhaav. vol. iv. No. 615, not. (*a*), & No. 619. not. (*c*).

In women, eſpecially ſuch as are pregnant, who uſe the intercoſtal muſcles more in ordinary inſpiration than men, the *pleura* will be more ſtretched at that time than during exſpiration; becauſe the cavity of the *thorax* is increaſed in wideneſs and depth, as well as length.

With regard to what the Doctor ſays of the ribs approaching each other in inſpiration; tho' this is certainly true of the ſuperior ribs, yet I have ſome doubt, whether it be ſo in the inferior ones: for, in a very full inſpiration, I can with my fingers plainly feel the ſix or ſeven inferior ribs recede from each other, and approach again in the ſucceeding exſpiration *. Wherefore it muſt appear, that the increaſe

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* The reaſon why not only the falſe ribs, but alſo ſome of the true ones, rather recede from than approach each other in inſpiration, may be underſtood from what is briefly ſaid concerning the motions of the *thorax*, by Mr. *Monro*, in his anatomy of the bones, edit. 5. p. 242.

of the pleuritic pain in time of inſpiration can be no proof, that the diſeaſe has not its ſeat ſometimes in the *pleura*.

UPON the whole, altho' Dr. *Haller*'s experiments ſhew, that ſeveral parts of animals are poſſeſſed of a more obſcure degree of feeling than has been commonly imagined; yet it is hoped, the reader will, after weighing what has been ſaid, be far from pronouncing them altogether inſenſible, or condemning the uniform opinion of phyſicians in all ages, concerning the parts which are affected in many diſeaſes, and, inſtead of it, embracing a doctrine which is far from being ſufficiently proved, and may, if made a foundation for practice, be of fatal conſequence.

PART II.

Of Irritability.

SECT. I.

ALTHO' many of the parts compoſing the human body are endowed with a conſiderable degree of elaſticity, whereby they reſtore themſelves when overſtretched; yet muſcular fibres alone are poſſeſſed of a proper contractile power, which is exerted, in conſequence either of an effort of the will, or of ſome *ſtimulus* applied to them, or their nerves: by the former, voluntary motion is produced; by the latter, involuntary*. The learned Dr. *Haller*, who chuſes to call the contractile power of irritated muſcles by the name of *Irritability*, has, by a variety

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* Vid. an Eſſay on the vital motions, *&c.* ſect. i. and x.

riety of curious experiments upon living animals, ſhewn, that it is a property of all muſcular fibres; and that no part, which is not muſcular, is irritable, altho', of the muſcular parts, ſome are more, and others leſs, ſenſible of irritation. But when, in his enumeration of the parts of the body that are or are not irritable, he allows irritability to the lacteal veins, mucous glands, and ſinuſes, and yet denies it wholly to the kidneys and ureters, and almoſt wholly to the arteries, veins, and excretory ducts of the glands, we cannot help differing greatly from him: ſince theſe laſt parts are, at leaſt, as much muſcular as the former; and ſince the Doctor's experiments on living and dying animals, ſhew neither the one nor the other to be irritable *.

THAT the ſmall arteries are not deſtitute of irritability, may be demonſtrated by undoubted experiments. Thus, when an acrid cataplaſm is applied

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* Act. Gottingenſ. vol. 2. p. 139.——143.

plied to the ſkin, or ſpirit of wine to the eye, Whence proceeds the inflammation which is ſoon produced in the ſkin, and almoſt inſtantly in the eye? Not, ſurely, from any increaſed force of the heart or larger arteries, but from the irritated veſſels themſelves, which are agitated with ſtrong alternate vibratory contractions; by means of which the moment of the blood in them is greatly increaſed, and red globules are puſhed into thoſe veſſels which, in a ſound ſtate, only receive *ſerum* or lymph.

NOR can we conclude that the arteries are deſtitute of irritability, becauſe the *aorta* was not obſerved to contract itſelf when pricked with a ſharp inſtrument, or touched with acrid liquors *; ſince the ſame is true of the mucous glands and ſinuſes, which yet our author allows to be irritable †. And it is not improbable, that the ſmall capillary arteries may be more irritable than the *aorta* or larger ones; becauſe their muſcular coat, as it is called, is much leſs firm and tendinous.

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* Act. Gottingenſ. vol, 2. p. 141.

† Id. p. 143.

Farther, Dr. *Haller* reckons the lacteal veins irritable, becauſe, after death, they contract themſelves ſo as to expell the chyle and become inviſible; * but do not all the arteries of the body, ſmall as well as great, alſo contract themſelves after death, and puſh moſt of their blood forward into the veins? And is not this coarctation of the lacteals owing more to the elaſticity of their coats now increaſed by cold, than to a proper muſcular contraction. However, if the lacteals be irritable, as is, I think, very probable, tho' for other reaſons than the one now mentioned; it will follow that the lymphatic and other veſſels of the body are ſo likewiſe: for the lacteals are only a kind of lymphatic veins ariſing from the villous coat of the guts, which, on account of the colour of their fluid, have got the name of *lacteal*. Nor have we any reaſon, from their muſcular ſtructure, to aſcribe irritability to the lacteals and thoracic duct, more than to the other veſſels of the body.

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* Act. Gotting. vol. 2. p. 142.

WITH regard to the veins, I ſhall only obſerve that, ſince the alternate contractions of the trunks of the *venæ cavæ* near the heart, ſhew them to be poſſeſſed of a remarkable degree of irritability; it is not probable that the other veins are *wholly* deſtitute of it. I know that Dr. *Haller* denies any proper motion to the *cava*, and aſcribes its ſeeming alternate dilatation to the blood puſhed back into it by the contracting auricle *. But, if this were true, how could the *cava* contract five or ſix times before the right auricle performed ſo much as one pulſation, as *Steno* has obſerved in rabbits †? or how could it poſſibly continue its alternate motions, not only for a conſiderable time after the right auricle had ceaſed to move *, but even after the heart, together with this auricle, was intirely ſeparated from it †? Theſe facts ſhew ſo clearly that the motions of the *venæ cavæ* do not proceed from the alternate contractions of the right auricle, as to make any farther obſervations on our learned author's miſtake, as to this matter, altogether needleſs.

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* Primæ lineæ phyſiolog. 2. edit. No. cxiii.

† Bartholin. epiſt. med. cent. iv. p. iii.

Does not the ſudden flow of pale urine in hiſteric caſes, and the increaſed derivation of *ſaliva* into the mouth of a hungry perſon from the taſte or even the ſight of grateful food, ſhew that the ſecerning veſſels of the kidneys and excretory ducts of the ſalivary glands are, in ſuch caſes, agitated with an unuſual oſcillatory motion, and conſequently not deſtitute of irritability?

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* Bartholin. epiſt. cent. iv. p. 110. and Eſſay on vital and involuntary motions, p. 354.

† Walæus de motu ſang. ad. fin. anatom. Bartholin. p. 783.

Nor ought Dr. *Haller* to have denied this power to the veſſels of the kidneys and excretory ducts of the glands: ſince he allows it to the lacrymal glands and mucous ſinuſes, becauſe they pour forth their fluids more copiouſly when ſtimulated; altho' his experiments diſcovered no ſigns of irritability in them[*].

WHEN a ſtone paſſes from the kidneys to the bladder, does not the irritation of the ſharp ſtone occaſion ſome kind of ſpaſmodic contraction in the *ureter*; and does not a large doſe of *opium* facilitate its paſſage, by abating or deſtroying the painful feeling, and conſequently leſſening the conſtriction of the *ureter?* This canal, therefore, ſeems to be poſſeſſed of ſome kind of irritability, notwithſtanding, Dr. *Haller* tells us, it was, in the animals he opened, inſenſible of the *ſtimulus* of oil of vitriol[†].

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[*] Act. Gotting. vol. 2. p. 143.

[†] Gotting. Act. vol. 2. p. 142.

If our author's experiments diſcovered no kind of irritability in the blood-veſſels, lacteals, glands and mucous ſinuſes, it will not follow that the *iris* is deſtitute of this power, altho' it did not appear to contract when irritated with a knife *.

The Doctor adds, that the dilatation of the pupil cannot be owing to any muſcular power becauſe it becomes wideſt at death or immediately after it †. I have elſewhere obſerved that the dilatation of the pupil is owing to the longitudinal fibres of the *uvea*, which by their natural contractility retract its edges, when the orbicular muſcle is not excited into contraction by the action of light on the *retina* ‡: at death, therefore, when the eye becomes inſenſible, the pupil muſt be very wide; but, ſometime after death, as the accurate *Winſlow* has always obſerved ‖, and I have alſo ſeen, the pupil becomes narrower, becauſe the longitudinal fibres of the *uvea* loſe their tone, become flabby, and are elongated. Nor does Dr. *Haller* ſeem to have attended to what is ſaid in page 111, and 129. of my Eſſay on the vital motions &c. when he mentions the dilatation of the pupil at death, as a clear proof that it is not owing to the contractile power of the fibres of the uvea; ſince this very dilatation of the pupil, compared with its coarctation ſome time after death, demonſtrates the truth of what I have advanced. But, after all, if the dilatation of the pupil be not owing to the elaſticity or natural contractility of the radiated fibres of the *uvea*, To what cauſe can it be aſcribed? For 'tis preſumed, our author has given up his notion of the aqueous humour preſſing the edges of the pupil outwards, as being contrary to the known laws of hydroſtatics. It may not, however, be improper to obſerve here, that, altho' we ſhould ſuppoſe the *uvea* to be, ſtrictly ſpeaking, not muſcular, but only a cellular membrane; yet, like the *dartos* of the *ſcrotum*, it would, by its elaſticity, retract the edges of the pupil as ſoon as the cauſe contracting it ceaſed to act. And altho', at the time of death, the pupil would, hence, be rendered very large, yet ſometime after it, when this cellular ſubſtance began to loſe its elaſtic power, the pupil would become narrower.

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* Act. Gotting. vol. 2. p. 143. † Ibid.

‡ Eſſay on vital motions, ſect. vii.

‖ Memoires acad. des ſciences 1721. edit. 8vo. p. 416.

Dr. *Haller*, becauſe he cannot diſcover any orbicular muſcle ſurrounding the edge of the pupil, concludes there is none; and aſcribes the contraction of this part to a ſtronger influx of fluids into the ſmaller veſſels of the *uvea*, occaſioned by the *ſtimulus* of light acting upon it. The inſufficiency of this hypotheſis we have ſhewn elſewhere *; and ſhall only add, that, as we conclude from the various motions of many of the ſmaller inſects, that they are, as well as larger animals, endowed with muſcles, tho' we can neither demonſtrate theſe inſtruments of motion by the anatomical knife, nor by the aſſiſtance of the microſcope; ſo we may infer the exiſtence of the orbicular muſcle of the *uvea* from the regular motions of the pupil, altho' its texture may be ſo delicate, as ſcarcely to be diſtinguiſhed by the anatomiſt from a denſe cellular membrane.

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* Eſſay on vital motions, p. 127, &c.

BUT, to return; there are ſome other things advanced by our learned author, in his account of the irritable parts of the body, which, tho' not ſatisfactory, we ſhall paſs over *; and proceed to conſider what he has offered concerning the nature of irritability.

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* Dr. *Haller* has repreſented me as ſaying, That the contraction of every muſcle of the body is interrupted with alternate relaxations (*a*): whereas in p. 20, 257, 260 and 261, of my Eſſay on the vital motions, I have expreſsly excepted the *ſphincter pupillæ*, muſcles of the

(*a*) Act. Gotting. vol. 2. p. 145.

SECT. II.

In my Eſſay on the vital and involuntary motions of animals, I had endeavoured to ſhew, that *ſtimuli* applied to the muſcles of animals excited them into

the internal ear, and ſome others, whoſe contraction is owing to a *ſtimulus* acting on ſome neighbouring or diſtant part. I have indeed affirmed, That thoſe muſcles to whoſe fibres a *ſtimulus* is immediately applied, are always agitated with alternate contractions and relaxations; nor do I know of one example to the contrary. 'Tis true, Dr. *Haller* has told us, That the bladder of urine is an exception from this rule; for, when it is prick'd with a knife, in a dog half-dead, it contracts itſelf, uniformly without any alternate relaxations, and expells the urine: (*a*) But, ſince *Wepſerus* has obſerved, that the bladder ſometimes contracts, of its own accord, after death, and expells the urine; it ſeems probable that, in our author's experiments, the contraction of the bladder and expulſion of the urine were owing more to the elaſticity of its coats, than to any proper

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(*a*) Act. Gotting. vol. 2. p. 142. and 145.

into contraction, by producing an uneaſy feeling in them or their nerves: but Dr. *Haller*, who thinks irritability an innate property of muſcular fibres,

proper muſcular action. He owns it only ſucceeded ſome times, *i, e,* we may ſuppoſe, when the impulſe communicated to the bladder by pricking it, together with its own elaſtic power, now increaſed by the diſtraction of its coats, were ſufficient to dilate the *ſphincter* and open a paſſage, for the urine, into the *urethra*; after which, the bladder would, merely by its elaſticity, expel this fluid, and reduce itſelf to its ſmalleſt bulk. And is it not to be preſumed, when the bladder contracts of its own accord after death and expells the urine, that this may be owing to the weight of the urine in certain ſituations dilating the *ſphincter*; and to the cold which, by its conſtringing power, may perhaps increaſe the contractility of the bladder, while its ſphincter, like all the other true muſcles of the body, is weakened and relaxed? In dead animals whoſe *abdomen* is not opened, the bladder may be ſo preſſed by the guts, which are generally much inflated after death, as to occaſion the expulſion of the urine.

contends, that it does not depend upon the nerves, and has no connexion with ſenſibility.

1. BECAUSE

BUT further, Dr. *Haller* muſt, in order to be conſiſtent with himſelf, give up his inſtance of the bladder; for he tells us (Act. Gotting. vol. 2. p. 139, and 144.), That all muſcles, not ſo much as one excepted, that he knows of, tremble and palpitate after death, and are alternately contracted and relaxed: if therefore the bladder of urine does not, when ſtimulated, contract in this manner, it will follow that it is not truly muſcular.

Add to this, that, if the uniform contraction of the bladder be a ſufficient proof of its muſcular ſtructure, we muſt allow the *dartos* or cellular membrane of the *ſcrotum* to be muſcular alſo; for it contracts uniformly and furls up the *ſcrotum*, when cold water and aſtringent or acrid liquors are applied to it, or when the ſkin of the *ſcrotum* is gently irritated by titillation.

Upon the whole therefore, it ſeems probable that the contraction obſerved by Dr. *Haller* in the bladder of urine, was not of the true muſcular kind. But, altho', from our learned author's experiments, one might be apt to conclude, that

1. **Because** the moſt ſenſible parts, ſuch as the nerves and ſkin, are not irritable [*].

2. **Because** the irritability of our organs is not obſerved to be in proportion to their ſenſibility [†]. And

3. **Because** parts deſtitute of feeling are irritable [‡].

With regard to the firſt of theſe; ſince muſcles are the only organs of the body, which, by their particular fabric, are fitted for motion, it is ſo far from being a wonderful diſcovery, as our author ſeems to think, That the nerves are deſtitute of irritability, that it is only a neceſſary conſequence of their make; for a power of contraction does not depend on ſenſibility alone,

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that the bladder itſelf is not truly muſcular, yet its *ſphincter* partakes undoubtedly of this nature; ſince, when irritated, it is agitated with alternate contractions and relaxations.

[*] Act. Gotting. vol. 2. p. 134.

[†] Id. p. 136.

[‡] Id. p. 134.

alone, but upon this in conjunction with a particular ſtructure.

THE proper anſwer therefore to this firſt argument, is, That, altho' irritability always infers ſome degree of ſenſibility, yet ſenſibility does not infer irritability, unleſs the part be, by its peculiar fabric, fitted for motion, *i. e.* in other words, unleſs it be what we call muſcular.

ALTHO' the ſkin is not irritable in the ſame ſenſe that the muſcles are, yet the inflammation and pain raiſed in it by bliſters and other acrid applications ſhew, that it is very readily fretted or irritated by *ſtimuli*. The ſkin, when ſtimulated, is not brought into alternate contractions, becauſe it is not by its ſtructure made capable of this kind of motion; but it becomes red, is inflamed, and pours forth its liquors ſo copiouſly, as to ſeparate the ſcarfſkin, and raiſe it in the form of a bladder filled with water, becauſe the ſmall veſſels, of which it is in a great meaſure compoſed, partake of a muſcular nature, and are, like the larger muſcles, excited into alternate contractions by *ſtimuli*.

FURTHER, the *dartos* or cellular membrane of the *ſcrotum* is contracted uniformly, when expoſed to the cold air or other *ſtimuli*; and the ſkin, from the application of cold air or water, ſeems likewiſe to ſuffer ſome kind of contraction, by which means it is raiſed into tubercles reſembling the ſkin of a gooſe. When cold water is ſuddenly, and without one's knowledge, applied to a part of the body that is warm, there is excited inſtantly a kind of ſhivering which ſpreads over the whole body; and not only the pores of that part to which the cold water was applied, but alſo of the whole body, are conſtricted. Do not theſe examples ſhew that the *dartos* and ſkin are affected by *ſtimuli*, and conſequently irritable, tho' not in the ſame ſenſe that the muſcles are? The irritability of the parts of the human body therefore, may perhaps be not improperly diſtinguiſhed into three kinds: *viz.* That power of alternate contraction, which is peculiar to thoſe organs, we call muſcles; that uniform conſtriction which happens to the *dartos* and pores of the ſkin; and that redneſs and inflammation which is excited in every part of the body that is ſenſible, as often as acrid things are applied to it; altho' indeed this laſt is only an effect of the firſt kind of irritability in the ſmall veſſels of the parts.

As to the ſecond argument, *viz.* That irritability is not obſerved to be in proportion to ſenſibility, our author has been very unlucky; ſince an inflammation of any irritable organ, which increaſes its ſenſibility, is always obſerved to make it more irritable, as will be ſhewn afterwards by a variety of examples. The Dr. however, in proof of his aſſertion, tells us, that the ſtomach is more ſenſible than the inteſtines,

teſtines, and yet leſs irritable; and that the heart itſelf is endowed with no acute feeling, and, when touched in a living perſon, occaſions fainting rather than pain *.

THE ſtomach has a particular feeling whereby it is very diſagreeably affected by things, that, as far as we can judge by our taſte or ſmell, have very little acrimony: it is the principal ſeat of hunger; and, as it is affected with a more diſagreeable ſenſation, when we have wanted food for any conſiderable time, than the guts, ſo likewiſe it is more ſenſible of an agreeable feeling from grateful food; and in theſe reſpects, it may be ſaid to be more ſenſible than the inteſtines. But, notwithſtanding this, the inteſtines ſeem to be as ſuſceptible of pain as the ſtomach, or indeed any other organ of the body: an inflammation in them is as painful, if not more ſo, than in the ſtomach; and jalap, *ſenna*, and other ſmart purgatives, which ſeldom occaſion any pain in the ſtomach, often affect the guts with ſevere gripings.

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* Act. Gotting. vol. 2. p. 136.

WITH regard to the heart; Dr. *Harvey* ſeems too haſtily to have concluded it to be void of feeling, becauſe the young Nobleman, whoſe heart he touched, ſcarce felt any thing at all: for, what this great man put his fingers to, was not the ſubſtance of the heart itſelf, but an inſenſible *callus*, or fungous fleſh covering and defending it. The truth of the matter is, that, as the ſkin, altho' one of the moſt ſenſible parts of the body, feels no pain from a ſlight preſſure or attrition, becauſe it is defended by the inſenſible *epidermis*; ſo the heart, when gently touched, feels little, becauſe it is covered with the inner *lamina* of the *pericardium*, which, like other membranes of the body, has but a ſmall degree of ſenſibility [*]. In like manner, the external ſurface of the inteſtines is rendered leſs ſenſible than it would otherwiſe be, by their being involved in the meſentery; and hence it is, that the woman mentioned by *Peyerus* felt no pain when her inteſtines were handled by him and *Wepferus* *. But, altho' the outer ſurface of the heart and inteſtines may have no great degree of ſenſibility, it will not thence follow, that their internal ſurface, where the natural *ſtimuli* exciting their motions act upon them, is not endowed with a more exquiſite feeling: nay the contrary is highly probable, if not altogether certain. Doctor *Haller* himſelf has obſerved, that the heart is much more affected in animals dying, or newly dead, by the gentle *ſtimulus* of warm water or air puſhed into its ventricles, than by applying the moſt acrid liquors to its external ſurface, or even pricking it with the point of a knife †; and it will appear from an

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[*] Act. Gotting. vol. 2. p. 130.

experiment

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* Parerg. anatom. exercitat. 1. cap. iv.

† Act. Gotting. vol. 1.

experiment to be mentioned afterwards, that, in ſome caſes, the *ſtimulus* of the blood within the cavities of the heart will excite a tremulous motion in this organ, when oil of vitriol applied to its external ſurface has not the leaſt effect this way.

WITH regard to the comparative ſenſibility and irritability of the heart and inteſtines, it is not eaſy to ſay any thing certain, nor is this needful; ſince from our author's experiments it does not appear clearly, whether the heart or inteſtines are moſt irritable *. The motions of the heart are indeed ſtronger and more frequently repeated; but thoſe of the inteſtines continue, in many animals, as long, if not longer, after death.

As for our author's third argument, *viz.* that parts deſtitute of feeling are irritable; there is not ſo much as one inſtance given, nor indeed can be given of a part being irritable that is naturally inſenſible and deſtitute of nerves *: but what he thinks equivalent to this, is, that muſcles continue to be irritable, not only for ſometime after their nerves have been tied or cut, and ſo all communication between them and the brain intercepted; but alſo after they have been intirely ſeparated from the body. And, indeed, it muſt be owned, there is a great deal of ſeeming weight in this argument: but that it is, nevertheleſs, inconcluſive, has been already ſhewn in the laſt ſection of my *Eſſay on the vital and other involuntary motions*

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* Act. Gotting. vol. 2. p. 147.

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* Our author indeed mentions, upon the authority of *Lupſius*, the ſecundines and membranes of the *ovum* as irritable, and yet deſtitute of nerves. But, if irritability, as he himſelf allows, be a property of muſcular fibres alone, it will follow, that the membranes of the *ovum*, which are not muſcular, cannot be irritable: but, ſuppoſing they were both one and the other, it is not a clear point, that they may not be ſupplied with ſmall nervous filaments propagated to them by means of the navel-ſtring.

motions of animals; and will, I hope, appear ſtill more ſo from the following conſiderations.

1. Altho' the irritability of muſcles continues, in a ſmall degree, for ſome time after their nerves are tied or otherwiſe deſtroyed; it will not follow, as our author thinks, that this power does not depend upon, or proceed from the nerves: for, if this were ſo, one would expect that, in a living animal, where the muſcles are all ſupplied with blood by the arteries, they ſhould continue to preſerve their power of irritability, not only for a few minutes, but for many hours and days after their nerves have been tied or cut. Further, if the irritability of the muſcles were not owing, ſome how, to the nerves or their influence, why ſhould a *ſtimulus*, applied to the nerves or *medulla oblongata*, produce ſuch remarkable convulſions?

These convulſions cannot be owing to the propulſion of any ſubtile fluid in the nerves, towards the muſcles; ſince, as Dr. *Haller* and others have obſerved[*], theſe motions follow equally whether a nerve going to any muſcle is ſtripped upwards or downwards. If they were owing to the connexion or vicinity of the nerves to the muſcles, one would expect, that more remarkable convulſions ſhould follow from an irritation of the tendons, than of the nerves: the contrary of which, however is true; for, while the irritation of a nerve produces ſtronger convulſive motions in the muſcles, than ariſe even from the laceration of their own fibres, the tendon, however pricked or irritated, produces no change in them[†]. The reaſon is plain; the tendon has little or no feeling, while the nerve has a very acute one.

But further, it ought to be obſerved, that when, after decollation, a frog's ſpinal marrow is deſtroyed with a red hot wire, no viſible motion is produced

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[*] Act. Gotting. vol. 2. p. 136. [†] Ib. p. 140.

produced in its limbs, or body, by pricking, cutting, or otherwiſe hurting them: only, when the ſkin of the thighs was diſſected off, and the muſcles were irritated, their fibres were agitated with a weak alternate tremulous motion. Now, as the ſtrong convulſive motions, excited by irritation in the legs and trunk of the body of a frog after decollation, are certainly to be aſcribed to the integrity of the ſpinal marrow, ſince they ceaſe as ſoon as it is deſtroyed; Is it not highly probable, that the weak tremulous motion in the irritated muſcles of a frog's thighs, after the deſtruction of the ſpinal marrow, were owing to the influence or power of their nerves, which ſtill remained intire *? It ſeems alſo to

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* As the alternate motions of the heart, in many animals, continue for a long time after the deſtruction of the brain and ſpinal marrow; Is it not probable, that its nerves are ſo conſtituted as to make its moving power leſs dependent on immediate ſupplies from the brain and ſpinal marrow, than that of the voluntary muſcles?

to deſerve particular notice, that, after the deſtruction of the ſpinal marrow, altho' the fibres of ſuch muſcles as were irritated, exhibited a weak tremulous motion; yet there was no ſympathy between the different muſcles, or other parts of the body, as was obſerved while the ſpinal marrow was intire: from whence it ſeems to follow, that the nerves diſtributed to the ſeveral parts of the body, have no communication but at their termination in the brain or ſpinal marrow; and that to this, perhaps alone, is owing the conſent or ſympathy obſerved between them.

Upon the whole; the weak alternate motions, produced by irritating muſcles whoſe nerves have been tied or cut, by no means prove, that their irritable power is independent of the nervous influence: they only ſhew, That theſe motions are not owing to any new derivation of ſpirits from the brain into the muſcles at that time; that the preſence of the nervous influence in its fibres is only requiſite; and that the ſpirits remaining in the nerves, below the ligature, and in the muſcular fibres, may be ſufficient to preſerve a certain degree of irritability, or power of motion in them, for ſome little time.

Dr. *Haller* further concludes irritability to be independent of the brain and nerves; becauſe the ſmalleſt inſects, which have no head, are irritable *: but, if this argument was good for any thing, it would prove ſenſibility and voluntary motion to be alſo independent of the brain and nerves; for the ſmalleſt inſects ſeem to be endowed with feeling, and undoubtedly perform voluntary motions. May not theſe inſects which want a head, have ſomething to ſupply the place of a brain, from which the nerves may take their riſe? Or may not the nerves be ſo formed in them, as to be ſufficient of themſelves, without a brain, for the purpoſes of motion and ſenſation? Arguments of this kind, which are drawn from our ignorance of the true ſtructure of animals, can be of no weight.

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* Act. Gotting. vol. 2. p 156.

2. Doctor *Haller*, while he denies feeling to the *dura* and *pia mater*, allows it to the medullary ſubſtance of the brain *; becauſe, when it is wounded, the muſcles of the body are convulſed in an extraordinary manner. Now, if the ſenſibility of the medullary part of the brain in living animals may be deduced from the convulſive motions which enſue upon hurting it: Are we not (our author himſelf being judge) to aſcribe feeling to the brain, even in animals newly killed; ſince in theſe the motion of the heart is renewed by irritating the *medulla oblongata*, and the whole muſcles of the body are convulſed by diſſecting the ſpinal-marrow †? And altho', in

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* Act. Gotting. vol. 2. p. 130. et 134. et primae linae Phyſiolog. 2d edit p. 238.

† Kauu impet. faciens, No. 330. et 333.

animals newly dead, the convulſive motions produced by irritating the *medulla oblongata* or *ſpinalis* be weaker and leſs remarkable than in living animals, yet it will by no means follow, that they are not indications of ſenſibility, and owing to the ſame cauſe as in living animals: for, as the death of the body, in general, ſoon puts an entire end to every kind of feeling and activity in the parts of moſt animals, ſo it is not to be doubted, that, immediately after death, theſe powers begin to be weakened; wherefore the motions owing to them muſt be leſs remarkable.

FURTHER, if the convulſions occaſioned by irritating a nerve in its natural ſtate are allowed by all to be a proof of its feeling, the like, tho' weaker, convulſions excited in the muſcles by irritating a cut or tied nerve muſt be an equal proof of its ſtill retaining, in ſome meaſure, its ſenſibility. When all communication, therefore, with the brain, by means of the nerves, is cut off, convulſive motions, which ariſe from a *ſtimulus* applied to any part, are equally a proof of the ſenſibility of that part, as if the communication were preſerved. And, if in the latter caſe, theſe motions may be juſtly aſcribed to the nerves, being hurt by the irritation, they muſt be equally ſo in the former.

3. But here it will be objected; How can there be any ſenſibility or feeling in a nerve, whoſe communication with the brain is cut off?

In anſwer to which it may be ſufficient to ſay, That, ſince we have ſtrong reaſons for believing that the parts of many inſects continue to be ſenſible for a conſiderable time after they have been divided from each other*; and that the bodies of ſome larger animals continue to live and feel after they are deprived of their heads[*]: Why may not the muſcles of men preſerve ſome degree of ſenſibility for a few moments after their nerves are tied or cut, altho' we may not be able to account for this, from any thing we know of the nature of the body, or of the manner in which the ſoul is preſent with, or acts upon it[†]?

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* Flies copulate and lay eggs after decollation. *Boyle*'s uſefullneſs of experimental philoſophy, part 2. pag. 16.

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[*] Vipers continue for three days after being deprived of their head and heart, to be manifeſtly ſenſible of punctures, and move their bodies, when pricked, juſt as entire vipers do. *Boyle*'s uſefulneſs of experimental philoſophy part 2. p. 16.

[†] If I were allowed to indulge myſelf in conjectures concerning a matter of which I know very little, I would ſay, that, altho' there can be no feeling or perception in the brain when a nerve is pricked below where it is cut or tyed, yet, if the ſoul be preſent every where in the body, as ſeems highly probable, there may be ſome kind of feeling or ſenſation excited in the nerve itſelf, which may

Redi tells us, that the head of a viper will bite half an hour after it is cut off from the body, (*vid. Jacobæi obſervat. de ranis et lacert. p.* 58.); and

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be ſufficient to produce a motion in the muſcles to which it belongs.

Dr. *Stuart* has produced ſeveral arguments to prove, that the inferior extremity of every nerve is to be conſidered as the brain of the organ or part in which it terminates; and that the ſoul is not confined to the brain or any part of it, but is preſent every where in the body, equally in the extremities of the nerves, as at their origin. (Diſſert. de motu muſcular. cap. v.) And if this be ſo, as it may for any thing that can be ſhewn to the contrary, Why may not a muſcle, whoſe nerve is tied or cut, continue, for ſome little time, ſenſible and irritable? Its ſenſibility will not indeed be attended with what is properly called *conſciouſneſs*, as diſtinguiſhed from *ſimple ſenſation*; becauſe this reflex act, by which a perſon knows his thoughts or ſenſations to be his own, is a faculty of the ſoul only exerciſed in the brain, with which all communication is now cut off.

As the ſoul ſeems to imagine, judge, reaſon, and remember in the brain only; ſo, why may

I have often obſerved, that a frog's head, after being ſeparated from its body, not only continued, for above half an hour, to move its eye-lids, noſtrils,

it not have, in the various other parts of the body, ſuch feelings or powers as are neceſſary for carrying on their ſeveral functions? Particularly, why may it not, in the muſcular fibres, have the power of ſimple ſenſation and of beginning motion? Or, which will amount to much the ſame thing, while the rational ſoul acts only in the brain, there may perhaps be, as ſome have thought, a ſentient active principle, which enlivens the whole body, and which continues to actuate the parts, for ſome time after their communication with the brain is ſtopt, *i. e.* as long as they continue in due order for being acted upon by it.

The more probable opinion, however, ſeems to be, that the ſoul is equally preſent in the extremities of the nerves thro' the whole body as in the brain. In thoſe, it is only capable of feeling or ſimple ſenſation; but in this, it exerciſes the powers of reflex conſciouſneſs and reaſon. When the communication of any part with the brain is cut off, the ſimple ſenſation of feeling excited in ſuch part is no longer perceived ſtrils, and muſcles of the lower jaw, when its brain or the ſkin of its head was touched with a probe, but ſometimes moved its eyes and eye-lids, when

ceived by the ſoul in the brain; and therefore is not attended with reflex conſciouſneſs: the nerves being now alſo deprived of the influence which uſed to be tranſmitted to them from the brain, ſoon become unfit to perform their functions; hence the powers of ſimple ſenſation and motion in the part, if it be muſcular, ceaſe by degrees, till at laſt it becomes quite dead. The communication, therefore, between the ſeveral organs and the brain, is not only neceſſary to preſerve their nerves, by means of ſome influence tranſmitted to them, in due order for performing their functions, and being properly affected by their ſeveral objects, but alſo, that the ſoul, as a conſcious and rational being, may be acquainted with theſe impreſſions.

It will be unfair to object here, that we aſcribe the intelligent powers of the mind to the bodily organs: for as the beſt muſician cannot make a flute give the ſound of a violin, nor a harpſicord that of a French horn, nor without theſe ſeveral inſtruments produce their ſounds and notes at all; in like manner, the nothing touched it, and as it were of its own accord; ſo that, without an obſtinate degree of ſcepticiſm in this matter, we cannot deny that the head continues to be animated for a conſiderable time after it is ſeparated from the body, and to perform not only

involuntary

ſoul, in the *preſent* ſtate, can only exerciſe its rational powers in the brain; it can only taſte in the tongue, ſmell in the noſe, ſee in the eyes, hear in the ears, and feel hunger in the ſtomach. But altho' the imagination, memory, and rational powers, depend upon the brain; yet the brain does not imagine, remember, or reaſon: altho' taſte depends on the tongue, ſmelling on the noſe, ſeeing on the eyes, and hearing on the ears; yet theſe organs neither taſte, ſmell, ſee, nor hear, but only that living ſentient principle which animates them.

It may be proper to obſerve, that, whether theſe conjectures, which are offered with a great deal of diffidence, ſhall be thought probable or not, the argument concerning the irritable power of the muſcles of animals will not be materially affected; ſince this muſt be determined, not by metaphyſical reaſonings, but by experiments and obſervations. Vid. Sect. iv. below.

involuntary motions when ſtimulated, but, in appearance, alſo voluntary ones. In like manner, the body of a frog, after being divided from the head, preſerves the power of motion, for above an hour; and when its hind feet or toes are cut, or otherwiſe hurt, the muſcles of its thighs, legs, and trunk are ſtrongly contracted, by which it raiſes its body from the table, and ſometimes moves from one place to another. When the muſcles of the thighs are pricked or cut with a knife, they are excited into contraction; but neither they, nor the neighbouring muſcles, are near ſo ſtrongly convulſed as when the toes are wounded: Whence ſhould this happen; and why ſhould not the muſcles of the legs and thighs be more ſtrongly convulſed, when they themſelves are wounded, than when the toes are treated in the ſame manner? This would undoubtedly be the caſe, if the motions of irritated muſcles were owing to ſome property of the inſenſible matter compoſing them. But, if, as we imagine, they are all to be derived from feeling, it is eaſy to ſee that, as the feet and toes are more ſenſible of pain when wounded, than the muſcles of the legs or thighs, ſtronger convulſions muſt be occaſioned by an irritation of the former, than of the latter.

FURTHER, we muſt either allow that both the head and body of a frog continue to be animated, for a conſiderable time, after they are ſeparated from each other; or elſe affirm that the life, feeling and active powers of animals, are, merely, properties of that kind of matter of which they are compoſed. The former opinion, is attended with ſome difficulties, which ariſe *ſolely* from our ignorance of the nature of immaterial beings: the latter, is inconſiſtent with all that we know of matter or its properties. If we admit it, therefore, we not only aſcribe qualities to matter which it does not poſſeſs, but preſume to limit, by our ſcanty and inadæquate capacities, the powers of incorporeal natures, their manner of acting upon bodies, and coexiſting with them.

If the ſoul were confined to the brain as many have thought *, Whence is it that a pigeon not only lives for ſeveral hours after being deprived of its brain, but alſo flies from one place to another †? And to what cauſe are we to aſcribe the continuance of life and motion, in a viper for three days after its head is cut off, and in a tortoiſe for three weeks after decollation, and ſix months after the loſs of its brain ‡? The motions performed by theſe animals cannot, ſurely, be attributed to their material part alone; unleſs we ſhall deny them a ſoul altogether, and, with *Des Cartes*, refer all their actions to their corporeal machinery. The very ingenious Dr. *Hales* writes me, that, having, many years ſince, tied a ligature about a frog's neck, to prevent any effuſion of blood, he cut off its head, and, thirty hours after, obſerved the blood circulating freely in the web of the foot: the frog alſo at this time moved its body when ſtimulated: but, on thruſting a needle down through the ſpinal marrow, the animal was ſtrongly convulſed, and, immediately after, became motionleſs.

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* Act. Gotting. vol. 2. p. 153.

† Baglivi opera 4to. prefat. p. xi.

‡ Redi. obſervat. circa animal. vivent. p. 209. &c.

If then the ſoul in pigeons, frogs, vipers and tortoiſes, is by no means confined to the brain, but can continue for a long time to actuate their bodies independent of it; and if, in many inſects which have no brain, every part of the body is both ſenſible and irritable[*]; why ſhould we deny, that, in man and ſuch animals as reſemble him moſt, the parts may continue to be actuated by the ſoul or ſentient principle for ſome few minutes after their communication with the brain has been cut off*?

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[*] Act. Gotting. vol. 2. p. 138.

If any man of ordinary ſenſe, who is no philoſopher, be aſked, Why the heart of a frog beats when ſeparated from the body, and renews its motions when pricked; he will readily ſay, It is becauſe there is *life* in it: and this is a proper anſwer; nor can a better, perhaps, be given by the ableſt philoſopher. If then *life* in animals be owing to the energy of a principle diſtinct from matter and of powers ſuperior to it, we have reaſon to conclude, that, as long as any ſigns of life remain in the bodies of animals or any of their parts, this principle ſtill continues to actuate them.

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* The difference between men and thoſe animals which live long after decollation or the exciſion of their heart, ſeems to be, that the latter are ſo framed that freſh ſupplies of blood and ſpirits from the heart and brain are not immediately neceſſary to keep the ſeveral parts in due order to be acted upon by the ſoul; as ſeems to be, in a great meaſure, the caſe in man and many other animals.

There are two kinds of motion from irritation, obſervable in living animals: *viz.* where the muſcle or organ itſelf is ſtimulated, and where the *ſtimulus* only affects ſome neighbouring or diſtant part. The firſt (of which kind is the motion of the heart) ſeems to be owing to the ſoul or ſentient principle as acting in the part moved; but the ſecond, to the ſoul as perceiving and acting in the brain: and of this kind is, the motion of ſneezing from an irritation of the noſe, and the contraction of the diaphragm in vomiting and in a *teneſmus* or ſtrangury. In order to the firſt kind of motions, an immediate communication with the brain is not abſolutely neceſſary, but only ſuch a ſhare of the nervous power in the muſcle or its nerves, as may be requiſite to fit its fibres for being acted upon by the ſoul or ſentient principle. But the caſe is quite otherwiſe in the ſecond; where the motion produced is through the intervention of the brain, and not by any *ſtimulus* applied to the part moved. And hence it is that, in an animal newly dead, the diaphragm is not brought into contraction by lacerating or pricking the *inteſtinum rectum* or neck of the bladder, altho' the fibres of theſe parts themſelves may be, thence, agitated with ſome tremulous motions. In like manner, tho' the muſcular coat of the ſtomach is excited into contraction ſome time after the death of the animal, by irritating it; yet the diaphragm is no way affected by this irritation: which, however, it would have been, if the animal had been alive. Agreeably to this, when any of the muſcles of a frog's legs are irritated ſome time after cutting off its head, almoſt all the muſcles belonging to the legs and thighs are brought into contraction, if the ſpinal marrow be entire: but, as ſoon as this marrow is deſtroyed, altho' the fibres of ſuch muſcles as are themſelves ſtimulated are affected with a weak tremulous motion, yet the neighbouring muſcles remain altogether at reſt.

I have elſewhere endeavoured to ſhew, That the ſuppoſition of the ſoul or ſentient principle's continuing for ſome time to actuate the ſeparated parts of animals, does not infer its *real* diviſibility *; nor is it neceſſary to repeat the ſame things again: but, I cannot help obſerving, that, when Dr. *Haller* repreſents me as holding the ſoul to be diviſible, ſo as that it may be cut into as many pieces as the anatomiſt pleaſes †; he charges me with an opinion which I not only do not maintain, but which I have brought arguments to diſprove. I ſhall only add, that the indiviſibility of the ſoul does not depend on the unity of the body, but on its own particular nature.

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* Eſſay on vital motions, &c. p. 380. &c.

† Act. Gotting. vol. 2. p. 137.

It muſt be acknowledged, that there is a great deal of obſcurity in theſe matters: but, as, in every part of nature, we find abundance of myſteries, as often as we puſh our inquiries to any great depth; it can be no wonder if we meet with difficulties, almoſt inſurmountable, in accounting for the motions of animals, or tracing them up to their firſt ſource: for, if we are far from underſtanding the communication of motion and other actions of *matter* upon *matter*, How ſhall we be able to comprehend the manner in which an *immaterial* principle acts upon it? But, as we can, from the little we know of matter, ſee that inactivity is one of its eſſential properties, we are hence convinced of the neceſſity of aſcribing the life and motions of animals to the power of an *incorporeal* Agent.

If we knew the manner of exiſtence of the ſoul, or the way in which it acts upon or is preſent with the body; it would be a very proper objection to any phyſiological opinion, that it was inconſiſtent with what we certainly knew of theſe things: but, as we are utterly ignorant of them, it is highly unreaſonable and abſurd to argue againſt an opinion ſupported by experiment and analogy, from its ſuppoſed inconſiſtency; with what? why, truly, with nothing! For what we are totally ignorant of, is, to us, as if it were nothing; and we can neither affirm nor deny any thing to be either conſiſtent or inconſiſtent with it.

SECT. III.

Doctor *Haller*, after endeavouring to prove that irritability is independent on ſenſibility, gives it, as his opinion, That this remarkable property of the muſcles, has its ſeat in the glutinous matter connecting the earthy elements of which their fibres are compoſed*; and that irritability ought to be looked upon as a particular property of this glutinous ſubſtance, in like manner as gravity is allowed to be a property of matter in general, altho' its cauſe cannot be aſſigned†.

But ſurely the glutinous matter of the muſcles of animals ſeems as unlikely to be endowed with an active power, ſuch as irritability, as any other conſtituent part of the animal body; nor can any thing be deduced from its endeavouring to ſhrink or ſhorten itſelf when drawn out‡; for the glue of the ſkin, ligaments and tendons, as well as of the muſcles, has this property, which is, indeed, a kind of elaſticity||, and no

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[*] Act. Gotting. vol. 2. p. 152.

[†] Ibid. p. 154. and 157.

[‡] Act. Gotting. vol. 2. p. 152.

[||] Elaſticity is not a property of hard bodies alone, as Dr. *Haller* ſeems to think (p. 152.), but is alſo found in ſoft ones: thus air, wool and the down of feathers are remarkably elaſtic.

way ſimilar to that power of alternate contraction which muſcular fibres are endowed with.

THE Doctor, in proof of his notion of the irritable nature of the muſcular glue, adds, that young animals which abound moſt in it are moſt irritable. The obſervation is certainly true, but proves nothing in the preſent caſe; for the ſkin, ligaments, and tendons (which laſt are a continuation of the muſcles, only harder and more compacted) abound much more in *glue* than the muſcles, and yet are not in any degree irritable. The greater irritability of the fibres of young animals is to be deduced from their greater ſenſibility, and this is owing to their greater ſoftneſs and tenderneſs: thus, what in new-born animals is a ſenſible and irritable muſcle, becomes afterwards a tendon, which, in a ſound ſtate, is deſtitute of irritability, and endowed with little or no feeling *.

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* Act. Gottingenſ. vol. 2. p. 140.

But farther, ſince the gelatinous matter in our aliments, and even in our blood, is quite deſtitute of the property of irritability, it muſt owe this power to the particular diſpoſition or arrangement of its parts, or to ſome other change which it ſuffers, when it becomes a part of a muſcle: And if this may be ſo, why may not the finer aud more ſubtile parts of the blood be ſo changed in the brain as there to acquire a power of feeling and thinking, *i. e.* if *irritability* be a property of the muſcular *glue*, why may not ſenſibility and intelligence be properties of the medullary ſubſtance of the brain? for the known properties of matter give us reaſon to think, that real activity is not more conſiſtent with its nature, than feeling or thought.

But it has been ſaid, that irritability may be a property of the muſcular glue as well as gravity is a property of matter in general: let us therefore conſider this notion a little, and ſee whither it will lead us. Gravity, which is a property of matter, continues to be ſo, let matter be ever ſo much altered or changed by fire, menſtruums, or other cauſes; but, when the *gluten* of the muſcles is extracted from them, it appears as inert and deſtitute of active powers as any other matter; nay, tho' allowed to remain in them, yet, in moſt animals, it loſes its power altogether very ſoon after the muſcles are ſeparated from the body.

But, ſuppoſing irritability to be a property of the muſcular glue in the ſame ſenſe that gravity is a property of all matter, yet, as the moſt attentive conſideration of the nature of matter has convinced philoſophers that gravity is not eſſential to it, but owing to ſome general cauſe acting upon it; ſo the irritability of the muſcular glue muſt be allowed not to be a property eſſential to it, but ariſing from the action of ſome other cauſe upon it. Gravity has been aſcribed either to the immediate and continued operation of an *immaterial* being, or to the action of ſome ſubtile elaſtic *medium* on matter: But, ſince the elaſticity of the parts compoſing ſuch a *medium* muſt be, at laſt, referred to the active power of ſome *incorporeal* cauſe, it follows, that gravity muſt be ſo likewiſe.

It appears therefore, after all that has been ſaid to ſhew that the motions of irritated muſcles are owing to a property of irritability in them or their glue, that we are at laſt obliged to refer them to the active power of an *immaterial* cauſe; unleſs we ſhall, contrary to all ſound philoſophy, chooſe to aſcribe feeling and proper activity to matter. And, as *gravity* muſt finally be reſolved into the power of that Being who upholds univerſal nature; ſo it is highly probable, that the irritability of the muſcles of animals is owing to that living ſentient principle, which animates and enlivens their whole frame.

SECT. IV.

HAVING thus endeavoured to lay open the inſufficiency of Dr. *Haller's* theory of irritability, we ſhall conclude with a few obſervations, which, if they do not demonſtrate, make it, at leaſt, extremely probable, that the motions of ſtimulated muſcles proceed from their ſenſibility or are cloſely connected with it: but, previous to theſe, muſt be allowed to take notice, that the word *irritability* ſeems to imply a kind of life or feeling in the part endowed with it, which renders it capable of being fretted, provoked or irritated, and therefore ſeems to be improperly applied to expreſs the contractile power of ſtimulated muſcles, if this power has no connection with, or dependence on their ſenſibility.

lity. We never talk of irritating a ſtone, a piece of wood, a tree, or indeed any thing that is deſtitute of feeling. *Irritability*, therefore, in the common acceptation of the word among mankind, implies ſome kind of ſenſibility; nay Dr. *Haller* himſelf, notwithſtanding his profeſſed deſign is to ſhew irritability to be independent of ſenſibility, ſpeaks once and again of parts that are not irritable, as not feeling or perceiving the acrid matter, or other *ſtimulus* applied to them *. So true is the obſervation of the poet,

> *Naturam expelles furca; tamen uſque recurret.*

BUT to return;

I. WE almoſt always obſerve the irritability of the muſcles and organs of the human body to bear a proportion to their ſenſibility. Thus, in young children, where the tender nerves and fibres are more eaſily hurt, and all the feelings are more exquiſite; the quickneſs of the pulſe and the violent convulſions with which they are often affected, from very ſlight cauſes, ſhew their muſcles to be endowed with a greater degree of irritability, than thoſe of adults *. In like manner, grown people of delicate nerves and very quick feelings are ſubject to ſpaſms and convulſive motions of their ſtomach, guts, *&c.* and to palpitations of their heart, from ſuch ſlight cauſes as would ſcarce ſenſibly affect men of firmer conſtitutions and leſs moveable nerves.

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* Acta Gottingenſ. vol. 2. p. 142.

On the other hand, in old people in whom all the feelings become leſs acute, the muſcles are leſs irritable; witneſs the ſlow motion of their heart. And, in apoplectic and comatous caſes, where the ſenſes are greatly impaired, the heart's motion, and that of reſpiration,

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* It may alſo be obſerved, that the parts of young animals which are moſt ſenſible, are not only moſt irritable, but retain their power of motion longeſt after death, or ſeparation from the body. Vid. Eſſay on vital motions, p. 358.

ſpiration, are remarkably ſlow; and the *ſtimulus* of the *fæces* is not ſufficient to bring the guts, diaphragm, and abdominal muſcles into contraction, as uſual.

FURTHER, the nerves, which are the moſt ſenſible parts of the body, produce, when irritated, the moſt remarkable convulſive motions in the muſcles; and, when they are, by being ſtretched, rendered more ſuſceptible of pain, an irritation of them produces ſtill greater convulſions *.

II. WHATEVER increaſes the ſenſibility of the muſcles or moving organs of our body, alſo increaſes their irritability.

THUS, when the ſtomach is inflamed, the mildeſt liquors, received into it, are apt to provoke vomiting, or the hiccup; whereas, in a ſound ſtate of this organ, brandy, vinegar, and other acrid liquors, produce no ſuch effect. When the neck of the bladder is ſlightly inflamed or excoriated, the urine, which uſed to give little diſturbance till collected in large quantity, irritates this tender part, ſo as to produce violent and often-repeated efforts to empty the bladder.

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* Act. Gottingenſ. vol. 2. p. 136.

When the *fauces* are attacked with an inflammation, the muſcles of deglutition are more ſtrongly convulſed in ſwallowing, than when theſe parts are in their natural ſtate. When the guts are by any means deprived, in a good meaſure, of their *mucus*, or rendered more ſenſible by a very ſlight degree of inflammation in their inner membrane; the gentleſt purgatives often operate as ſeverely as the ſtronger ones do in a perſon in perfect health. When, without any degree of erection in the *penis*, the *ſemen* eſcapes into the *urethra*, the *muſculi acceleratores urinæ* are no way affected by it: but, as often as the *penis* is erected, and thereby its parts rendered more ſenſible, and, as it were, half inflamed, the *ſemen* is no ſooner poured into the beginning of the *urethra*, than the above-mentioned muſcles are excited into ſtrong convulſive contractions.

The heart becomes ſo irritable, when itſelf or the *pericardium* is inflamed, as to be agitated with violent convulſions and palpitations. Nay, the tendons, which, in a ſound ſtate, have little or no feeling, and are not irritable *, become, when inflamed, ſo ſenſible of *ſtimuli*, that the moſt violent convulſions have been occaſioned by pricking, tearing, or otherwiſe irritating them.

A diſagreeable ſenſation in the ſtomach from wind, relaxation of its coats, and other cauſes, quickens (eſpecially in people whoſe nervous ſyſtem is very delicate and moveable) the motion of the heart; which will be often rendered ſlower again, by a glaſs of generous wine, a dram of brandy, or any thing that, by invigorating the

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* Act. Gottingenſ. vol. 2. p. 140.

ſtomach, baniſhes the uneaſy ſenſation in it.

A diſagreeable feeling in the ſtomach renders the heart more irritable, becauſe, by means of its nervous ſympathy with this organ, it increaſes its ſenſibility; and, in like manner, an inflammation or unuſual irritation in the kidneys or inteſtines increaſe the irritability of the ſtomach: but how a diſagreeable feeling in the ſtomach ſhould immediately alter the nature of the *gluten* of the fibres of the heart, in which Dr. *Haller* places the irritability of this organ, is as inconceiveable, as it is inexplicable upon any juſt principles of phyſics.

If therefore it appears, that the irritability of the moving organs of our body is increaſed as often as their own ſenſibility, or that of other parts with which they have a remarkable ſympathy, is increaſed; it will be thought, at leaſt, highly probable, that the irritability of any part depends upon its ſenſibility.

III. Whatever leſſens or deſtroys the ſenſibility of the muſcles of animals, alſo leſſens or deſtroys their irritability or power of motion.

Thus, when one's fingers or limbs have been long expoſed to ſevere cold, they not only become inſenſible, but paralytic. Frogs, bats, and other animals, with numbers of the inſect-tribe, are ſo benumned by the winter's cold, as to be deprived of all feeling and motion: their blood does not circulate, nor their hearts beat; and their muſcles, tho' torn, cut in pieces, or otherwiſe ſtimulated, are not brought into contraction.

During the time of incubation, the chick's heart is obſerved to beat faſter or ſlower, and with more or leſs force, *i. e.* to become more or leſs irritable, as it is expoſed to greater or leſs degrees of heat; nay, after its motion has been ſtopt altogether by cold, a gentle heat will make it, in a very ſhort time, begin to contract anew *.

FURTHER, this *punctum ſaliens*, or heart of the chick, which, when touched with any thing capable of hurting it, is excited into quicker and ſtronger contractions, after being expoſed for ſome time to too great cold, is not affected by the moſt powerful *ſtimuli*.

IT appears, therefore, that feeling and irritability are deſtroyed by cold, and reſtored by a proper degree of heat, and are ſo cloſely connected together, that the latter is never to be found where the former is totally wanting.

IF authority could be of any weight in a matter which is to be determined by experiments and obſervations, we might ſupport our opinion with the name of one of the moſt judicious and ſucceſsful inquirers into nature, that any age has produced. "Ego pluribus "experimentis certus ſum (*ſays the il-* "*luſtrious* Harvey), non motum ſolum-

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* Harvey de generat. animal. exercit. xvii.

" modo puncto ſalienti ineſſe, ſed ſen-
" ſum etiam; nam, ad quemlibet, vel
" minimum, tactum, videbis punctum
" hoc varie commoveri, et quaſi irrita-
" ri.—Vidi, inquam, ſæpiſſime, aliique
" qui una mecum aderant, ab acûs, ſty-
" li, aut digiti contactu, immo vero a
" calore aut frigore vehementiore ad-
" moto, aut cujuſlibet rei moleſtantis
" occurſu, punctum hoc varia ſensûs
" indicia, pulſuum nempe varias per-
" mutationes, ictuſque validiores ac fre-
" quentiores, edidiſſe; ut non dubitan-
" dum ſit quin punctum hoc (anima-
" lis inſtar) vivat, moveatur, ac ſen-
" tiat." De generat. animal. exerc. xvii.

Upon occaſion of quoting Dr. *Harvey*, it may not be improper to take notice of the error of thoſe who ſeem to think the irritability of the muſcles a late diſcovery*. If by *irritability*

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* *Tiſſot. diſcours preliminaire ſur l'irritabilité* prefixed to his tranſlation of Dr. *Haller's* treatiſe of the ſenſible and irritable parts.

tability is meant that power by which muſcles contract when they are pricked, fretted, or otherwiſe ſtimulated, 'tis plain this was not unknown to *Harvey*; and many authors ſince his time might be named, who have particularly mentioned it †. But, if by *irritability*

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† The irritability of the heart, after being ſeparated from the body, has been generally known to phyſicians and philoſophers for near a century paſt. And *Swammerdam* tells us, that, in diſſecting animals alive, he obſerved contractions, not only in the muſcles, but in every muſcular fibre, tho' ſeparated from the body of the muſcle. Tract. de reſpiratione, cap. vii. § v. 1667.

Dr. *Gliſſon*, in his book *de ventriculo et inteſtinis* (1677.), has ſeveral chapters on the irritability of the parts of the body; where he not only mentions the heart and inteſtines as endowed with this property, but tells us particularly, that the fibres of the muſcles in dead animals are brought into contraction when acrid liquors are applied to them. cap. vii. No. 3. He gives ſeveral examples of the irritability of parts from ſympathy, and mentions the cauſes which may produce either too ſmall *bility* be meant an active property of the muſcular glue analogous to gravity; this, it muſt be confeſſed, is a new diſcovery, tho' not likely to prove a laſting

ſmall or too great a degree of irritability in the fibres. cap. ix. No. 4, 5, 6, & 7. He ſuppoſes irritability to ariſe from a natural perception in the fibres, without which they could be no more affected by any irritating cauſe than a deaf man is by ſounds. This natural perception he diſtinguiſhes from feeling, concerning which he reaſons at great length, but with little perſpicuity. cap. vii.

Peyerus, after endeavouring to confute *Harvey*'s opinion of the chick's heart being not only endowed with motion, but alſo feeling, and aſcribing the irritability of this organ to its exquiſite but unknown ſtructure; adds, "Conſtat vero piſcium plurimos, nec non "inſecta et alia quædam animalcula motus "ſui aut vitæ admodum eſſe tenacia, adeo ut "in partes quoque diſſecta ſeſe aliquamdiu "adhuc motitent, imprimis ſi adhibito ſtimulo, "inſuper laceſſantur. Parerg. anat. med. "7mum pag. 200. Genev. 1681." The irritability of the inteſtines and heart was ſo well known to *Bohnius*, that he deduces the periſtaltic laſting one. *Opinionum commenta delet dies, naturæ judicia confirmat.*

But, to return from this digreſſion; *Opium*, which is remarkable for its power

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tic motion of the inteſtines from the irritation of the aliment, and aſcribes the alternate contraction of the heart, *partly* to the *ſtimulus* of the blood ruſhing into its cavities, which had been mentioned before by *Harvey* and *Gliſſon* as the ſole cauſe of the heart's motion. Circul. anatom. phyſiolog. p. 105. & 163. edit. 1686.

Baglivius has, in his book *de fibra motrice*, an intire chapter *de irritatione ſolidorum ſive ſtimulis et variis ſtimulorum effectibus:* from which it appears, that he was far from being ignorant of the power of *ſtimuli* to excite the parts of living animals into contraction. He has alſo ſeveral experiments concerning the irritability of the heart after being ſeparated from the body, and mentions particularly that frogs are convulſed by punctures an hour after they have been deprived of all the *viſcera* of the *thorax* and *abdomen*. Exper. xi. *de circulatione ſanguinis in rana.*

Among the later writers, Dr. *De Gorter* has, in many places of his works, taken notice of the motions of ſuch parts of animals as are irritated; and obſerves, that theſe motions are not power of impairing or deſtroying the ſenſibility of all the parts of the body, alſo leſſens or ſuſpends the irritability or moving power of the muſcles. Thus,

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not to be accounted for from elaſticity. "Sed "præterea cum omnes fibræ nervoſæ vellicatæ "ſeſe inordinate et involuntarie moveant, pa-"tet minimam cauſam ſæpe ſufficere ad totam "corporis œconomiam turbandam.— Cur au-"tem a vellicatione pars aliqua nervoſa ſtatim "contrahitur, difficile explicatur; veritas au-"tem ejus aſſerti ubique manifeſta eſt, non "modo in nervo iſto vellicato, ſed et in reli-"quis ſurculis nerveis ab eadem origine veni-"entibus, ut in ſternutatione, tuſſi, vomitu, &c. "Sentio id eſſe adſcribendum ſummi Opificis "placito, qui voluit corpus noſtrum ita concin-"nare, ut ſtatim ac vellicetur pars nervoſa, "ibidem demandentur ſpiritus; hoc enim ab "elaſticitate partium derivare, vellicatione vel "ſtimulo agitatarum et oſcillantium fruſtra "tentarunt multi." *Gorteri compend. medicinæ*, p. 58, & 63. Lugd. Batav. 1735.

Mr. *Monro*, in his anatomy of the nerves, tells us, "That all muſcles, but eſpecially "the heart, continue to contract, in an irre-"gular way, for ſome time after they are "cut away from the animal to whom they be-"longed;

Thus, in a ſmall doſe, it puts a ſtop to vomiting and coughing, and quiets the convulſive motions of the *inteſtinum rectum*, bladder, abdominal muſcles

and

“ longed; and that, after this motion of “ theirs has ceaſed, it may be reſtored again by breathing upon them, or pricking “ them with a ſharp inſtrument.” Anatomy of the human bones and nerves, p. 38. edit. 3d, 1741.

Dr. *Haller*, ſpeaking, a dozen of years ago, of the motion of the heart in time of ſleep, ſays; “ Cæterum tota theoria iſta ſimpliciſſimo “ phænomeno, a nemine negabili, nititur, “ omnem fibram muſculoſam animalis vivi, “ irritatam a quacunque cauſa, continuo in “ contractionem ire, ita ut hæc ipſa ultima “ nota ſit qua animalia imperfecta a vegetabilibus dignoſcantur.” And afterwards, with regard to the motion of the heart after its ſeparation from the body, he expreſſes himſelf thus; “ Omnino videtur quod alibi “ faſſus ſum, cum PRÆCEPTORE, in fibra “ animali aliquam ad irritationes contractilitatem ſupereſſe, quæ ſimplici elatere fortior, “ a motu muſculari diverſa, quod cerebri cordiſque non indiga ſit, et in ipſa hujus fibræ

“ humidæ

and diaphragm in a *teneſmus* and ſtrangury, altho' the *ſtimuli*, which produced theſe motions, continue to act on

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"humidæ adhuc et integræ fabrica fundata "eſſe videtur." Boerhaav. prælect. academ. vol. iv. p. 586, & 616. 1743.

Dr. *Winter*, in 1746, publiſhed an oration *de certitudine in medicina practica*, wherein, it is ſaid, he has referred all the motions of the human body to the irritable nature of the fibres and the power of a *ſtimulus*; acknowledging, however, with *Baglivi*, the *dura mater* as the fountain from which all our motions ſpring. But this piece I have not yet had the good fortune to ſee.

In an eſſay on the vital and other involuntary motions of animals publiſhed in 1751, the author, after conſidering particularly three kinds of contraction obſervable in the muſcles of animals, *viz.* natural, voluntary, and involuntary from a *ſtimulus*, endeavours to ſhew, that all the vital and involuntary motions are owing to ſome *ſtimulus* irritating either the organs moved, or ſome part with which they have a particular ſympathy; that the alternate contractions, excited in muſcles by irritating ſubſtances applied to them, proceed from their ſenſibility, and are no more than an on the parts: when given in much larger quantity, it ſuſpends the periſtaltic motion of the guts, and makes the heart contract more ſlowly, till

being

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an effort of nature to throw off what is hurtful: from which he concludes, that, if the ſenſibility of the muſcles be not a property of the matter of which they are compoſed, but owing to a ſuperior principle animating them, all the vital and other involuntary motions muſt ultimately be aſcribed to the active power of this principle.

Laſtly, Dr. *Haller*, in his treatiſe *de partibus corporis humani ſenſibilibus et irritabilibus*, publiſhed in the *Gottingen* tranſactions for 1752, has, by a great many curious experiments, proved, not only that all muſcular fibres, and them alone, are endowed with irritability or a power of alternate contraction, but has alſo ſhewn, that ſome muſcles and organs are poſſeſſed of this power in a greater degree than others. He has further endeavoured to prove, that the irritability of the muſcles is independent of the nervous power, and has no connexion with ſenſibility, but is owing to the glutinous matter of the muſcular fibres.

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being by degrees rendered quite inſenſible, its motion ceaſes altogether.

But, as Dr. *Haller*, who allows that *opium* deſtroys the irritability of the ſtomach, inteſtines and other muſcles, denies it to have any power over the heart, * and ſeems to call in queſtion thoſe experiments of mine which ſhew, that *opium*, injected into the ſtomach and guts of frogs, renders the motion of the heart much ſlower than uſual, and at laſt puts a final ſtop to it †; I thought it neceſſary to endeavour to clear up this matter by ſome farther experiments, which I ſhall here briefly relate.

From what has been ſaid, together with the ſhort hiſtory of irritability given by Dr. *Haller*, (Act. Gotting. vol. 2. p. 154, &c.) it appears, that the contractile power of ſtimulated muſcles has been long known to phyſicians, tho' within theſe few years paſt, it has been made the ſubject of more particular inquiry.

* Act. Gotting. vol. 2. p. 147, 154. & 157.

† Eſſay on vital motions, p. 370, *&c.*

(α) *June* 5th 1755, at 18 minutes paſt four in the afternoon, I injected a turbid ſolution of half an ounce of *opium* in eight ounces of water, into the ſtomach and guts of a frog; and, as it ſquirted out moſt of the ſolution injected by the *anus*, I threw in ſome more in its place. At 24 minutes paſt five the ſame evening, I opened this frog, and obſerved the heart beating very ſlowly, not above ſeven times in a minute; when it was touched with the point of a pair of ſciſſars, it renewed its motion faſter for two or three pulſations; after which it became as ſlow as before. The other muſcles of this frog were not at this time brought into contraction by pricking or tearing their fibres.

(β) I laid open the whole *abdomen* and *thorax* of a frog; and, at 28 minutes paſt ſeven in the morning, immerſed it in a turbid ſolution of *opium*, *viz.* the ſame that was made uſe of in the preceeding and following experiments. At forty minutes after ſeven, I turned the frog on its back, and obſerved its heart beating between ten and eleven times in a minute. Having laid it again on its belly, that it might be more expoſed to the action of the *opium*; at forty eight minutes paſt ſeven, I turned it again to its back, and obſerving the heart without motion, I opened the *pericardium*; which producing no effect, I cut the heart out of the body, and laid it on a plate, when it gave two or three pulſes, and never after moved, altho' it was pricked once and again with a pin.

(γ) I cut off a frog's head, and intirely deſtroyed its ſpinal marrow by puſhing a ſmall probe down through the ſpine, which occaſioned ſtrong convulſions of all the muſcles, eſpecially thoſe of the inferior extremities. Ten minutes after this, I opened the *thorax*, and found the heart beating 45 times in a minute. Sixteen minutes after decollation and the deſtruction of the ſpinal marrow, it moved 40 times in the minute. After half an hour, it made 36, and, after fifty minutes, only 30 pulſations in the minute, which were now become very ſmall and feeble.

N. B. When the *thorax* of another frog was opened immediately after decollation and the deſtruction of its ſpinal marrow, its heart beat 60 times in a minute.

(*δ*) I cut out the heart of a frog, and put it into fountain-water, at twenty three minutes paſt twelve. After twelve minutes immerſion, I took it out of the water, when it beat 20 times in a minute. Having immerſed it for five minutes more, it ceaſed from motion; and when taken out of the water, did not move except when pricked, and then only performed one pulſation.

(ε) Eight minutes paſt eleven, I cut out the heart of another frog, and immerſed it in fountain-water. 28 minutes after eleven, it continued to move: but its motion, tho' at the rate of eleven pulſations in thirty ſeconds, was confined to about one third of the heart next its *apex*. Two minutes after this, obſerving it without any motion, I took it out of the water, and laid it on a table, where it remained at reſt, unleſs when touched. Soon after this, however, it began to move, and, at 25 minutes after immerſion, performed 9 pulſations in 63 ſeconds.

(ζ) I cut out the heart of a frog, and, at 32 minutes paſt ten, immerſed it in a turbid ſolution of *opium* in water of the ſame degree of heat with the fountain-water uſed in the two laſt experiments *. After this heart had been immerſed ten minutes, I took it out of the ſolution, and laid it on a table; but it made not the ſmalleſt motion: and when pricked with the point of a knife, tho' it quickly recovered its ſhape, yet it was not excited into a proper contraction as the heart of δ. I continued

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* *Viz.* nearly 60 degrees of *Farenheit's* thermometer.

to obſerve this heart from time to time for above half an hour, but it never made the leaſt motion.

(*η*) I cut out the heart of another frog, and put it into the ſame turbid ſolution of *opium*; after ſeven minutes immerſion, I took it out, and laid it on a plate, where it remained at reſt. When pricked with a knife, it did not perform a full pulſation, but ſeemed to feel a little, by a very faint kind of motion which was excited in ſome of its fibres.

(*θ*) Mr. *Robert Ramſay* ſtudent of medicine, at my deſire, made the following experiment. After making an opening into the cavity of the *abdomen* of a ſmall dog near ſix months old, he injected by the wound a dram of *opium* diſſolved in two ounces and a half of water; but, before he could ſtitch up the wound, about an ounce of the ſolution eſcaped. Four minutes after making the injection, he laid bare the *thorax*, by diſſecting off the teguments, which did not ſeem to give the dog any pain; and could plainly feel the motion of his heart through the *pleura*. It beat 76 times in the minute, but became gradually ſlower *. Immediately after counting the pulſe, Mr. *Ramſay* cut the ribs on each ſide of the *ſternum*, which he laid back in the uſual way. The heart, which was thus brought into view, appeared quite turgid, and continued in motion about five minutes: during which time it performed only between 60 and 65 weak vibrations; for they were not compleat contractions. While the heart was thus moving, warm ſpittle was firſt applied to it, then cold water, and, laſt of all, oil of vitriol, which ſhrivelled the parts it touched, almoſt in the ſame manner as a hot iron would have done; but none of them accelerated the heart's vibrations, which became gradually ſlower, till they ceaſed altogether.

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* This dog's heart, in a natural ſtate and before the injection, beat 150 in the minute.

Agreeably to this experiment, we are told by Dr. *Alſton*, in his learned diſſertation on *opium*, that a filtrated ſolution of this medicine in water, having been injected into the veins of a dog, his pulſe, which, when he was firſt ſeized with convulſions, was, rendered quick and ſmall, became afterwards full and ſlow *. And Dr. *Kaau Boerhaave* informs us, that in a ſmall dog, which he opened ten hours after he had ſwallowed three grains of *opium*, the motion of the heart and arteries was very ſlow †.

From theſe experiments it evidently appears, that, as *opium* deſtroys the ſenſibility of all the parts of the body, ſo it deprives the muſcles of all power of motion; nor does the heart, in this reſpect, poſſeſs any privilege above the other muſcles, except that its moving power is not ſo ſoon deſtroyed by *opium* as theirs.

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* Vid. Medical Eſſays, vol. 5. p. 1. art. xii.

† Cor lentiſſime movebatur. Motus in arteriis (ſcil. duræ et piæ matris) debilis et valde lentus. Vid. impet. faciens *Hippocrati* dictum. No. 433.

How Dr. *Haller* came to be ſo greatly deceived as to this matter, I cannot pretend to conjecture; ſince he has not told us in what manner his experiments were made: but, it is not to be doubted, that his candor and love of truth will make him readily acknowledge his miſtake, as ſoon as he ſhall diſcover it.

IV. WHEN a viper is pricked with the point of a knife three days after being deprived of its head, heart, and other *viſcera*; it moves, not only the muſcles whoſe fibres are touched, but alſo the other muſcles of its body which have no connexion with thoſe that are ſtimulated. This indicates either a ſympathy between theſe muſcles, which ſuppoſes feeling, or ſome general active principle animating them, which being affected with a diſagreeable ſenſation by the *ſtimulus* applied to any one muſcle, brings many others into action, in order to avoid what is hurtful to it. In like manner, when a few drops of boiling water fall on one's leg, the muſcles which ſerve to move this member, are inſtantly and involuntarily brought into contraction, in order to remove it from the offending cauſe.

A frog, after it has been deprived of its head, will, when touched, often jump and move about for a very conſiderable time; and it is obſervable, that, when the toes of its hind feet are any way ſtimulated, it conſtantly draws them up to its body; nay, if, when they are in this ſituation, the toes are again irritated, the legs and feet are not extended, but brought ſtill cloſer to the body. If one of the legs is pulled down from the body and kept in an extended ſtate; no ſooner are the toes of this foot wounded, than the leg is drawn up to the body as before. Now, if theſe motions were owing to ſome property of the inſenſible matter of which the muſcles are compoſed, Why ſhould not an irritation of the toes be ſometimes followed by a contraction of the extenſor as well as the flexor muſcles of the legs and thighs? But, if we allow them to be owing to the painful ſenſation in the toes, we ſhall ſee that the frog does, in this caſe, with its limbs, juſt what a ſnail does with its horns when they are roughly touched.

AGAIN, it is very remarkable, that, when the toes of a frog are pricked or otherwiſe wounded inſtantly after decollation, there is either no motion produced in the muſcles of the legs at all, or a very inconſiderable one. But, if the toes of a frog be touched with one's finger, ten, fifteen or twenty minutes after decollation, the legs and thighs are immediately drawn up to its body; and, if they be at this time wounded, pricked, or cut with a penknife, the muſcles, not only of the legs and thighs, but alſo of the trunk of the body, are, for the moſt part, ſtrongly contracted, and the animal ſometimes moves from one place to another.

Is not the irritation of the toes, immediately after decollation, rendered ineffectual to produce any motion in the muſcles of the legs and thighs, by the greater pain occaſioned by cutting off the head *? And are not the muſcles of the poſterior extremities, as well as of the trunk of the body, brought into action by wounding the toes fifteen or twenty minutes after decollation, becauſe the pain produced by cutting off the head is now ſo much leſſened (perhaps wholly obliterated) as not to prevent the animal from feeling very ſenſibly when its toes are hurt?

It were to be wiſhed that thoſe who chooſe to account for the irritability of the muſcles, not from their ſenſibility, but from ſome unknown property of the matter compoſing them, would,

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* **Duobus doloribus ſimul obortis, non in eodem loco, vehementior obſcurat alterum.** Hippocrat. Aphor. Lib. 2. No. 46.

would, inſtead of moving objections concerning the ſeat of the ſoul, its extenſion, diviſibility and manner of coexiſting with the body, favour us, if they can, with ſome probable explication of the *phænomena* above-mentioned.

V. THAT the motions of irritated muſcles are owing to the ſenſation excited by the *ſtimulus* applied to them, will appear highly probable, if we conſider, that we are, in fact, conſcious of many involuntary motions in our bodies proceeding from a particular ſenſation, either in the organs moved, or in ſome neighbouring part. This is the caſe with the motions of the ſtomach and diaphragm, in vomiting and the hiccup, of the great guts and diaphragm in a *teneſmus*, of the *acceleratores urinæ* in expelling the *ſemen*, and of the intercoſtal muſcles and diaphragm in ſneezing, coughing, and ſometimes even in breathing; nay, when, by ſudden fear or any great ſurprize, the heart is ſet a palpitating, we have a particular feeling in this muſcle, partly from the blood, ruſhing ſuddenly and in too great quantity into it. More examples might be given, but theſe may ſuffice to ſhew the connexion there is betwixt the ſenſibility and irritability of the moving organs of our body.

Upon ſuppoſition that the motions of irritated muſcles did not proceed from any kind of feeling, but from ſome inanimate cauſe, their contractions ſhould be all, either regularly alternate, or equable and uninterrupted, like the falling of the leaves of the ſenſitive plant * ; but we find, that, while moſt of our muſcles are brought by the action of a *ſtimulus* into alternate contractions, there are ſome few which contract uniformly and equably during the time the *ſtimulus* operates, without any intermiſſions or alternate relaxations. Of this kind is, the contraction of the diaphragm and abdominal muſcles when the *inteſtinum rectum* is irritated, of the *ſphincter pupillæ* while the ſame degree of light continues to act on the *retina*, and of the muſcles of the internal ear as long as the ſame ſound is applied to this organ. Nay, the diaphragm, which is brought into one continued contraction by a *ſtimulus* affecting the *inteſtinum rectum*, is agitated with alternate convulſions from an irritation of the left orifice of the ſtomach, or of the olfactory nerves. What account can poſſibly be given of this, upon ſuppoſition that theſe motions proceed from the *gluten* of the muſcular fibres? or what difference can it make to this inſenſible *glue*, whether the *ſtimulus* be applied to the noſe or *anus?* But, allowing theſe motions to ariſe in conſequence of an uneaſy ſenſation in the part ſtimulated, it will immediately appear, that they are performed in ſuch manner as is moſt effectual to leſſen or remove the irritating cauſe *.

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* I have elſewhere ſhewn by experiments, that the falling of the leaves of the ſenſitive plant, when touched, does not indicate any kind of feeling, and is no way ſimilar to the alternate contractions of irritated muſcles. Eſſay on vital motions &c. p. 245.

Again, if the motions of muſcles from a *ſtimulus* were not owing to a feeling, How could the convulſive motions of the diaphragm in the hiccup be often immediately ſtopt by ſudden fear, joy, or grief? Why ſhould an irritation of the olfactory nerves become ineffectual to produce ſneezing, when ſome of the muſcles of the back or *thorax* are affected with a rheumatiſm? And why ſhould the convulſive motions of the ſtomach and diaphragm in vomiting, be frequently interrupted by extraordinary fear, or any very great and ſudden ſurprize? It will be difficult, nay impoſſible, to give any ſatisfactory ſolution of theſe *phænomena*, if the motions of irritated muſcles are ſuppoſed to proceed from ſome unknown property of their *inſenſible glue:* but they are at once intelligible and clear, upon ſuppoſition that they are owing to an uneaſy ſenſation; for as often as this feeling is overpowered by a ſtronger one in ſome other part of the body, or, when the mind is ſo ſuddenly and ſtrongly affected by external objects, as, for a ſhort time, to become almoſt inſenſible of the irritation, the motions owing to it muſt be leſſened or ceaſe.

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* Vid. An Eſſay on the vital and involuntary motions, p. 258, &c.

GRAVITY, magnetiſm and electricity, are all regular and uniform in their operations, and beſpeak nothing of feeling or life in the bodies which are endowed with them, and may therefore be ſuppoſed to proceed immediately from material cauſes; altho' the activity of theſe cauſes muſt be, at laſt, referred to the great ORIGIN of all power and life in the univerſe. But the motions of animal bodies from a *ſtimulus*, are, in many caſes, ſo plainly ly perceived to flow from an uneaſy feeling, their various *phænomena* can be ſo eaſily explained upon this ſuppoſition, and are ſo unaccountable on any other, that it is matter of no ſmall wonder to find many learned and ingenious Phyſiologiſts uſing their utmoſt efforts to overthrow this opinion, and ſtruggling, but in vain, to derive thoſe motions from inanimate matter.

LIFE, ſenſe and proper activity, ſeem to be inconſiſtent with the known properties of matter; wherefore, when we ſee a ſyſtem of matter endowed with theſe, we may, without preſumption, conclude, that they are owing, not to the material ſyſtem alone, but to ſome active principle animating it. And altho', even upon this ſuppoſition, it may be very difficult to account for ſome of the motions obſerved in ſuch a ſyſtem, or in its parts when ſeparated, we cannot hence conclude, that they are not owing to any ſuch power; but only that our ignorance of the nature of immaterial beings, and of their particular union with, and manner of acting upon bodies, throws a veil of obſcurity over theſe things, which the moſt enlightened philoſopher will never be able to remove.

Dr. *Haller*, towards the end of his performance, has thrown out ſome reflexions upon my manner of writing, and the few experiments I had made on dying animals; which, tho' it were eaſy to obviate, I ſhall paſs by unnoticed, from a conſciouſneſs of their being ill-founded, a diſlike of introducing any thing perſonal into a philoſophical diſpute, and a perſuaſion that the Doctor himſelf will not, upon a *cool* review, intirely approve of them.

FINIS.